THE

CHICAGO MEDICAL

JOURNAL & EXAMINER.

VOL. XLIII.—OCTOBER, 1881.—NO. 4.

Original Communications.

ARTICLE I.

IS CRANIOTOMY EVER JUSTIFIABLE? By A. REEVES JACKSON, A.M., M.D., etc. Read before the Chicago Gynæcological Society, December 17, 1880.

Mr. President and Gentlemen:—I regret that I am obliged to commence with an apology. I have to present you with a very brief and incomplete paper,—its brevity being, however, its greatest merit. But, thinking it better to do my duty partially than not do it at all, I have brought the paper before you "scarce half made up."

I ask your attention to the consideration of the question:

IS CRANIOTOMY EVER JUSTIFIABLE?

The act of parturition in the human female must always have been one of the most interesting character; and, although we have no facts upon which to base the opinion that the process was other than physiological and safe in the very early history of our race, it seems probable that many causes must have soon come into action which were certain to occasion anxiety and danger.

Out of the perils and difficulties which came to attend parturition arose the art of midwifery. And, as we might expect, this concerned itself chiefly with the cases in which delivery could not be completed without artificial aid. When the passage of the child became obstructed nothing could be more natural than to lessen its size, and extract it by means of the hand, hooks and pincers. Accordingly, we find that the earliest obstetrical operation of which we have any mention is that of craniotomy. This operation is spoken of very fully by Hippocrates and other early writers; and, inasmuch as both turning and the use of the forceps were unknown to them, craniotomy was probably practiced in many cases where these alternatives would have been employed at a later day.

Embryotomy holds the peculiar relation to surgery that it is the only operation which is undertaken and sanctioned by the medical profession with the avowed intention of destroying a human life. All other obstetrical operations—version, forceps, Cæsarean section and other forms of laparotomy—afford a chance of safety to both mother and child. Nevertheless, this apparently cruel practice has been justified upon what seem strong and proper grounds. Even now, however, there are some conscientious practitioners who hold that the mutilation of the child is only justifiable after its death has been positively ascertained.

The common sentiment upon this point is, that when the child, living or dead, cannot be removed from the mother by the more ordinary methods,—forceps or turning,—and the dimensions of the natural pelvis are such as to permit its delivery by lessening the size of the fœtal head, such mutilation is proper and commendable; and that the life of the child, under such circumstances, should be subservient to that of the mother. This doctrine is held and promulgated, with perhaps a very few exceptions, by the leading obstetricians throughout the world.

A few months ago a somewhat remarkable paper was read before the Obstetrical Society of Dublin, by Dr. R. J. Kinkead,* in which the author takes very decided ground in opposition to these views, and maintains that, if craniotomy be ever justifiable its proper sphere is extremely limited.

* Craniotomy and its Alternatives—Cæsarean Section, Laparo-Elytrotomy, and Porro's Operation. R. J. Kinkead, A.B., M.D.—*Obs. Jl. Gr. Br. and Ireland*, March, 1880.

It is my purpose, in this paper, to notice and briefly comment upon the arguments which have been advanced by the writer referred to in support of his position. In order to do this more conveniently, I have endeavored to embody his views in a series of propositions.

Proposition 1. *The child, whilst in its mother's womb, has a separate, individual existence; has as perfect a right to its life as its mother has to hers; that our duty binds us to put its life into the scale against that of a being like ourselves, accountable to the Almighty; that, in short, we have no right to entertain the question of the relative value of one human life as compared with another.*

A fallacy underlies this argument; for, while it is true, in the abstract, that one human life is as good as another, yet, when two lives are in imminent peril and one may be saved by the sacrifice of the other,—*and in no other way*,—it seems highly proper for us to choose which shall be rescued. And this principle of action has always been recognized in the case in question; scientists and philanthropists have agreed,—generally, at least,—that the life of the mother should be held superior to that of her unborn child, and that if one must perish, it should be the latter. It is conceded that we should preferably save the strong rather than the weak; the mother, who is bound by many ties, social and moral, to her family and the world, rather than the infant without hopes, fears, affections or responsibilities. So that, while we may admit that human life should be held at the disposal of God alone, we need not forget or ignore the fact that our reason has been given us for the purpose of affording us proper guidance in cases of doubt and difficulty. And when our reason and the experience of centuries teach us that the lives of two of His creatures are mortally endangered, and that we may save one only by prompt and efficient action, we feel justified in saving that which seems to our finite judgment the most valuable.

Proposition 2. *In consequence of the teaching that the safety of the mother is always to be preferred to that of the child, resort to embryotomy has been too frequent; and attempts have been made to perforate and deliver in pelves so extremely narrow that extraction was impossible, leaving no alternative but to either*

operate subsequently on the mother or let her die undelivered. Hence, such teaching is improper and dangerous.

The truth of this proposition may be fully admitted—and I think it ought to be—yet it is not so much an argument against the performance of craniotomy as against its improper use. Those who have had the best teaching are not necessarily the best taught. Any surgical operation or other remedial measure may be subject to abuse, without the fact militating against the proper and judicious use of it. It is quite conceivable—quite probable, indeed—that the attempts to deliver, which are characterized by Mr. Kinkead as unwarrantable, have not been the result of carelessness but the consequence of inherent difficulties in the case. For example, a pelvis may be sufficiently large to permit the extraction of a child of usual size after mutilation; and the attempt to deliver by craniotomy would, under the circumstances, be quite justifiable, even though it might have to be abandoned subsequently owing to the fœtus being larger than common.

Proposition 3. *In the case of a woman in labor, with such a degree of pelvic obstruction as to make it evident that unless speedy aid be given, both mother and child must die, it becomes our plain duty to save one of the two so endangered if we cannot save both; hence, craniotomy being necessarily fatal should not be performed unless we are assured that, with reasonable skill in its performance, the mother's life will be spared.*

This brings us to the consideration of the question, What are the smallest pelvic diameters which will permit the extraction of a dismembered fœtus with safety to the mother? Upon this point authorities differ; for, while some state that it cannot be accomplished unless there is a clear space of three and a half by two inches, the majority fix the limit at three by two, and three by two and a half. Barnes, indeed, states that under some circumstances three by one inch may be sufficient.

It is clear that the limit should be such as would be reasonably certain to secure the safety of the mother; for, as one author urges, if craniotomy be attempted in any case in which a secondary resort to another method must be had, the danger to the mother is enormously increased; and two lives may possibly be sacrificed in the attempt to save one. But, to make choice of

operation in many of these cases must be very embarrassing. It is very difficult, perhaps impossible, to ascertain within a fraction of an inch, the diameters of the pelvis, especially during labor, the time when they usually first come under notice. Then there are the considerable differences which may exist in the size of the fœtal head,—quite as important an element in the calculation, and one which we can only estimate. According to statistics collected by the late Dr. Parry, of Philadelphia, out of seventy cases of craniotomy where the smallest diameter was two and a half inches or under, the maternal mortality was about one in two and a half, nearly equal to the usual mortality from Cæsarean section.

In comparing the relative mortality of craniotomy and Cæsarean section,—the first being usually placed at one in five, and the latter at one in two and a half,—Mr. Kinkead lays great stress upon the fact that where craniotomy has been performed after long duration of labor—say forty-eight hours—the maternal mortality becomes greatly increased; and that where vain attempts to deliver have been made with forceps, it reaches a a mortality of one in two and a half, thus becoming as fatal as Cæsarean section. He proceeds, further, to show that while the tables relating to Cæsarean section show a mortality of about 57.6 per cent. they include cases in which the patients had been long in labor, some actually dying; others suffering from dangerous and deadly diseases; some in whom craniotomy had been tried and failed; and that, hence, a fair opinion as to the rate of mortality which would obtain under the best and most favorable circumstances was not at present attainable. He states, however, that in the few cases in which timely operations were performed the maternal mortality was reduced to 25 per cent. But he does not seem to consider that the tables upon which the mortality of craniotomy cases is based also include unfavorable cases,—cases in which labor has been of long duration, where the patient has been suffering from previous disease, and where unavailing attempts have been made to deliver by forceps, etc., and that if these were all included the operation would make relatively as favorably a showing compared with Cæsarean section as it does now.

Proposition 4. *In carcinoma of the uterus or vagina, a disease which must in a short time put an end to the existence of the mother, we are justified in saving the child, even though it be at the expense of some additional risk to the mother.*

Here the author makes a really strong point, and I coincide with him. The same principle which impels us to save the mother rather than the child under ordinary circumstances, should here urge us to reverse our action. We should save the best. Here, the child, if saved, would probably become a useful member of society, while the days of the mother are numbered. Embryotomy certainly destroys the child, and in this case does not save the mother; at best it could only for a brief time prolong a painful existence; and it has been shown by statistics that where craniotomy has been performed under these circumstances, it has been almost as dangerous to the mother as Cæsarean section. In reply to the argument, that because the mother's chance for living is small we have no right to still further lessen it, Mr. Kinkead aptly replies that this would be true if the child had no right to live.

Proposition 5. *When labor is complicated by the presence of uterine, ovarian or other pelvic tumors of considerable size, which are incapable of being removed or emptied of their contents, such dangerous pressure must be made upon them, even after the fœtal head has been reduced in size, that craniotomy should not be resorted to in preference to some of its alternatives—Cœsarean section, gastro-elytrotomy or Porro's operation.*

Here, too, I agree with Mr. Kinkead. Usually, in these cases, unlike those in which the deformity is bony, all the pelvic diameters are affected, and there is very great difficulty in extracting even the dismembered child. According to Stadfelt, the maternal mortality in such cases reaches 60 per cent., and when we consider that all the children are killed, the position taken by the author seems impregnable.

DR. JACKSON: In closing the discussion, I have but little to add to what has already been said. I want, however, to sincerely thank the members of the society for the very kind manner in which they have received and commented upon the paper in its present crude and unfinished form. I also thank them for

the courteous permission they have given me to revise and complete it at a subsequent time.

It seems well, in these days of changing views and practice, to occasionally come back to the consideration of old landmarks. In doing so we shall find that every change proposed and practiced is not necessarily an improvement. It was because I regarded the views of Dr. Kinkead, upon the subject of craniotomy, as revolutionary and unsound, that I thought it expedient to bring them before this society for the consideration of the latter; and I am pleased to find such a unanimous expression of opinion in favor of the old views and old and tried methods. To what extent the heterodox views of Dr. Kinkead have found sympathy and support in his own country I cannot say; but, I feel like congratulating this society on the fact that they have found no encouragement here.

ARTICLE II.

THE ACTUAL CAUTERY. By T. F. FRANK, M.D., Pittsburgh, Pa.

Within the meaning of the term actual cautery are embraced all methods by which animal tissues are destroyed, by contact with heated instruments, whether the heat is produced by fire directly, or by the aid of the galvanic battery; and the importance of the application consists in the quality of temperature, but not the mode by which it is produced. By the selection of galvanism for the heating of cautery instruments, however, we are better able to secure to them a more uniform temperature, essential for a successful issue of those operations, than can be done by any other process, and so far the galvanic battery has advantages. Its electric influence is no other than that obtained from metal heated by fire.

With all the advantages the electro cautery can be claimed to possess, it is highly overestimated, and not worth the money for an outfit.

During the past twelve years, I have had ample opportunities

to test all the so-called modern improvements in galvanic cautery apparatus in operations on cancers, polypi, hæmorrhoids, and in uterine surgery, but I must confess that many times, when my expensive fancy instruments failed me, I had to fall back upon a clumsy carbon-zinc cell made for me years ago, by an ordinary mechanic, at a moderate price.

Two years ago Mr. Isaac G. H., of Bradford, McKean Co., Pa., presented himself at my house for an operation on piles by galvano-cautery. The tumors were unusually large, and I feared that the Dawson battery I owned would fail me, as it had done in minor cases; and being unwilling to transport the large cell before mentioned to the hospital where the patient had taken a room, I obtained, through the kindness of Dr. D. N. Rankin, of Allegheny City, a Byrne battery, to help me out should the Dawson fail. Both the instruments were in good condition, and produced all the heat any manipulator could obtain from them; but had it not been for the most economical expenditure of the battery force, I could not have completed the operation. Much of the successful issue of this case depended upon the service I obtained through the assistance of Drs. Rankin and R. S. Sutton, which enabled me to give attention to the action of the batteries, which otherwise I would have been obliged to trust to more inexperienced hands.

Some time in July, 1880, I was requested by Dr. Willard, of Allegheny City, to remove for a patient of his, from Toledo, Ohio, the greater part of the neck of the uterus, on account of carcinoma involving the os. The doctor had just purchased a Piffard cautery, and wishing to test its capacity, requested me to use it in this case. I fortunately made some experiments with this battery, after having prepared it according to directions accompanying the instrument, or the operation would have been a failure. The knife to be employed became hardly warm, while I used the manufacturer's formula for acid, but subsequently I obtained better results, and had the patient prepared for the operation. With a far stronger solution than directed by the maker of the battery, I could heat the platinum knives very well, but was unable to maintain a good temperature during the operation, and there was considerable hæmorrhage to contend with.

I consider it bad practice to rely upon the actual cautery as an instrument for the removal of any growth to which access can be had by the knife, the scissors, or any other rapid method of extirpation. The surgeon does not contemplate dealing with the parts to be removed, but to bring the remaining tissues in the most favorable condition for recovery, and for that purpose the actual cautery becomes a most valuable remedy.

There are no tumors to which the actual cautery is applicable but what any ordinary surgeon may remove with the knife first, and control the bleeding temporarily, till the cautery can be applied. In cases of piles, cauterization is most eminently useful, and in all those cases the bleeding may be prevented by clamping the tumor before cutting it off, and before the cautery can be brought to action.

Now comes the consideration, whether for the cauterization of an open, accessible surface, we need the complicated and expensive paraphernalia connected with our electro-cautery instruments, or whether that operation cannot be more simplified? Every surgeon, no matter how obscure his practice, will find occasions to use the actual cautery, but few are able to purchase a battery, at an expense of $60—or $85 with the necessary cautery instruments—and at that obtain a very unreliable apparatus, probably never to be used after the first attempt.

Acting upon this suggestion, I have constructed a more simple cautery, which is in every particular reliable, and can be furnished for one fifth the cost, including the burner, for what any electro-cautery battery is sold without the handles, domes and knives.

For the benefit of those who wish to interest themselves in that particular practice, I give the following description of my new cautery, which I have named the Hydrogen Cautery: It consists of a simple closed copper and zinc element, so arranged internally that the hydrogen escaping from the negative element is retained inside the cell, and is rapidly acquiring density, till a pressure of three ounces to the square inch is obtained. By that time the zinc is free from the contact with the acid, and no more gas can be generated till a portion is allowed to escape for the purpose of cauterization.

The instrument has the capacity of one quart, and after being charged with eight ounces of water acidulated with four ounces

of common sulphuric acid, it will perform steady cautery work upon the most bloody tissues during one hour. The *modus operandi* of this cell, after the acid has been introduced, is perfectly automatic.

For the cautery I have constructed a small Bunson burner out of pipe clay, which with the full pressure of the gas will produce an almost invisible flame the size of a pepper-corn. The caliber of it can be reduced to that of a pin's head. The latter I adopt in most all of my operations.

Mr. William S. S., of Duke Center, Pa., was sent to me for an operation on piles, by Dr. Thos. Stuart, of Erie, Pa. He came at the time when I was making comparative experiments in the presence of several medical gentlemen with the Piffard and Dawson batteries and my hydrogen cautery upon raw beef. The experiment conclusively demonstrated that with one charge of my cell, I could cauterize more surface than the other two did unitedly. The two batteries had to be shaken and pumped constantly, and the acid liquid became boiling hot, while I had my cell in the side pocket of my coat, perfectly cool, and performing its work admirably. Mr. S. was operated upon the next day. He had five very large piles. After constricting the tumors by a snare clamp I lately constructed for those operations, I cut them down with a bistoury, and then simply brushed the little flame, held at a proper distance, over it, till the stumps were as dry and of the appearance of sponge. I never obtained as good results from a cautery operation before this. The patient recovered without requiring any attention after the operation, and left the city on a visit to his relatives in Michigan the sixth day.

For the more delicate operations on the cavities of the mouth and nose, where it becomes of great importance to confine the heat to a particular spot during a definite period, to prevent organs like the Eustachian tube and the middle ear from suffering from the heat radiated by the cautery, I have arranged an electric device, by which I can instantly send a small jet of heat, after the burner has been applied cold to the diseased part, under cover of a shell. This maneuver can be accomplished by the motion of one finger, and the heat stopped instantly in a like manner.

I give this paper with a desire to disincumber the actual cautery, and popularize one of the most promising surgical appliances.

Article III.

Vaccination: A Consideration of some Points as to the Identity of Variola and Vaccinia. By Thomas F. Wood.

The physician of the present day is no longer an anxious inquirer about the minute details of the theory and practice of vaccination. He regards all the difficulties as having been overcome, and he is willing to rest upon the glories which Jenner achieved. Notwithstanding the apparently settled state in which we find vaccination to-day, there are few more intricate studies underlying any subject than the present practice of vaccination.

The original works of Jenner, Woodville, Pearson, Bryce, Willan, Steinbrenner, Gregory, Bonsquet, Waterhouse, Redmond Coxe, and the hundred other carefully prepared volumes which from 1798 to 1843 were read and studied with so much anxiety, are fast disappearing from private libraries, and they are only known to the reader of to-day by the meagre extracts made from them, which are found in the current standard text-books. The literature of vaccination embraced within the dates mentioned above, marked the most brilliant era in medicine, and no richer field of study can be found to-day, than the treatises on vaccination prepared by so many careful writers. He who would be master of the vast questions which go to make up our knowledge of vaccination, cannot do so and neglect the original works of Jenner and his cotemporaries. Many of the principles there wrought out stand fast to-day. Many of the questions then anxiously propounded remain undetermined to-day. Many of the fast rules laid down by Jenner, and neglected by a few generations, we gladly return to eighty years after they are announced, because by disregarding them we have been led into error.

We wish we could feel as confident as Professor Lyman that recent studies of the nature of vaccine (Chicago Medical Journal and Examiner, July, 1881, p. 1) have given any material weight to the doctrine of identity. He thinks "we are now in

a position to form a clear idea regarding a subject in connection with which there is no longer any place for mystery."

He furthermore says, in the same article:

"The opinion was long entertained vaguely by many physicians that small-pox and vaccinia were entirely distinct, though similar, diseases. This belief was based upon the fact that Jenner did not himself fully comprehend the manner in which these two forms are related. He believed that they were identical in their origin, but he did not demonstrate that identity."

The opinion is still held by eminent vaccinologists of the non-identity of variola and vaccinia. Recent investigations upon the subject have tended to confirm the opinion that the diseases are structurally and clinically different. Surely, in the estimation of most excellent students, the beautiful experiments of the late lamented Ceely, as to the production of vaccinia by the inoculation of cows with variolous matter, are seriously doubted. These experiments by Ceely, of all the series now so famous in the history of vaccination, were the most reliable and convincing, although we think we can show that good reasons exist why we should regard them with distrust, if not reject them entirely. We contend that small-pox and cow-pox are not identical:

1. Because cow-pox has never been converted by any means into small-pox.

2. Because, in the light of the attempts made by many careful and truthful experimenters, to produce small-pox by the inoculation of the cow with small-pox virus, the number of failures on the part of such a very large majority of them, together with the numerous positive denials made of the possibility of success, it may be concluded that small-pox virus is never converted into vaccine by transmission through the cow. The results of Gassner, of Ceely, and Thielé, and Badcock are not left out of consideration in making this statement.

3. Cow-pox does not become epidemic,* nor does it spread by effluvia from the human subject. It is always confined to the

* An Inquiry into the Causes and Effects of Variolæ Vaccinæ. Edward Jenner, M. D., F. R. S., Springfield Edition, 1802, pp. 54 and 55. "It (cow-pox) has been conceived to be contagious among cows without contact; but this idea cannot be well founded, because the cattle in one meadow do not infect those in another, unless they be handled or milked by those who bring the infectious matter with them."

point of insertion in the human subject, never producing a general eruption of vaccine vesicles.

4. Structurally considered, the vesicles of vaccination and the pustules of inoculation are very dissimilar. Clinically considered, the course and duration of the two diseases are not unlike, but certainly not identical, having enough points of difference to enable the skilled diagnostician to differentiate them.

It becomes necessary, in order that we shall not do injustice to the experimenters who have claimed to produce genuine cow-pox by the inoculation of cows with small-pox virus, to go over the history of these attempts as briefly as possible.

The impulse given to these attempts was by Jenner's theory, that vaccine was but a species of small-pox. His nomenclature fully proves this: thus, he names the vaccine disease *variolæ vaccinæ*. Moreover, he fully enunciates his belief in the identity of small-pox and cow-pox, and his biographer says: * "He always considered small-pox and cow-pox as modifications of the same distemper; and that in employing vaccine lymph, we only made use of means to impregnate the constitution with the disease in its mildest, instead of propagating it in its virulent and contagious form." † There has been always a traditional opinion of the identity of cow-pox, founded upon the statements of writers, contemporaries of Jenner, or following closely after him. There was little originality in any of the volumes advocating this theory, many of them following Jenner's narrative precisely, adding only some cases coming under their observation.

None of the variolous inoculations of cattle for artificial cow-pox claimed to be successful ‡ until 1807, nine years after Jenner gave his "Inquiry" to the world,—the first attempt reported as being successful, being performed by Dr. Gassner, of Günzberg. The results were called in question, says Trousseau.§ Twenty-nine years later, Thielé, of Kasan, in Russia, announced successful production of artificial cow-pox. The small-pox matter em-

* Baron's Life of Jenner, p. 241.

† The theory of the transmission of grease from the horse to the cow was the favorite theory of Jenner. Inquiry, pp. 58 and 59.

‡ These inoculations had been tried in vain by Coleman, Ring, Sacco, Numann, Fiard, Bousquet, Dalton, etc. *Steinbrenner*. Traite Sur la Vaccine, p. 613.

§ Lectures on Clinical Medicine. Vol. II. English Edition, p. 115.

ployed in these inoculations was taken immediately from variolous pustules, while they were yet transparent, nacreous, pearly, and of which the lymph was still very limpid; or, better still, he used lymph which had been kept ten to twelve days between two pieces of glass.* Steinbrenner, in remarking on the results of Thielé, says: "Let us say furthermore, *en passant*, that this author, convinced by his experience of the analogy of the vaccine and variolous virus, had the idea of trying to mitigate the latter, in order to make it produce the same results as vaccine, and that without making it pass previously through the body of the cow. He pretends it will become so by leaving variolous virus for ten or twelve days between two glasses closed with wax; then he only dilutes it with tepid milk, and uses it thus prepared. He repeated the same thing with new lymph for many [series] following. The secondary fever did not show itself; the virus becomes so mitigated in the sixth generation that it produces nothing more than the symptoms of ordinary vaccine. It can then be employed without further precautions to vaccinate from arm to arm." (Steinbrenner, p. 615.)

It is very evident from the above statement that if Thielé's observations as to variolous inoculation are no sounder than his opinion of extracting the virulency from small-pox virus, that we must be exceedingly guarded how we accept his opinion. In the light of our present knowledge, it would be the height of folly to dare to go before the public with such an announcement. Small-pox virus cannot be tamed by such gentle means; you may destroy it utterly, but so long as there is any activity left it maintains its same contagious properties, and probably cannot be so diluted as to be harmless. It is true, there are degrees of small-pox. Some types are so mild as to appear harmless. I inoculated one patient with small-pox virus in 1865, and he had only two resulting pustules, although there was no evidence of vaccination, judging by absence of cicatrices. But can any one deny that an unprotected person coming in contact with the mildest cases of small-pox; one like this, for instance, would not be liable to small-pox in some form?

* Steinbrenner, p. 614.

Steinbrenner himself also failed to inoculate two cattle upon which he made the attempt, although he says that he was not then informed of the minutiæ of the process as detailed by Thielé and Ceely.

Dr. Ceely's cases are not so easily disposed of by merely denying his success. Dr. George Gregory * was willing to admit Ceely's success. "No doubt," he says, "can be entertained of their correctness." * * * "But "the lymph thus obtained has been called variolo-vaccine, to distinguish it from the idiopathic affection of the animal." "One effect of these experiments (of Ceely's) has been to refute Jenner's favorite notion, that the cow-pox is the parent of small-pox. So far as they go, they tend to show that small-pox is the primary, and cow-pox the secondary disorder. But it may be reasonably asked, do these experiments warrant the conclusion that cow-pox and small-pox are identical? To me it appears that they do not. The disorders are allied (so are measles and scarlatina), but they are not therefore identical. * * * Unlike small-pox, cow-pox produces no eruption, no constitutional disturbance; it throws off no contagious emanations. It can be perpetuated from man to man in a uniform state of intensity; whereas the inoculation of small-pox produces the disorder in varying shades of severity. The local characters of each malady are no less strikingly contrasted. The variolous action goes on to pustulation, to the *acumination* of the pustule, to sloughing of the corion, and implication of the subjacent cellular membrane. The vesicle of cow-pox never loses its umbilicated character; no purulent matter forms; the areola is circular, not irregular, like that of the inoculated small-pox."

The above quotations are made from this well-known master, who for years was physician to the Highgate Small-pox Hospital, and who was a teacher of recognized ability at St. Thomas' Hospital. These opinions were expressed only a few years after Ceely's famous experiments were published.

When we consider these inoculations in the light of our present knowledge of the natural history of cow-pox as observed in the large number of vaccinations directly from the cow, it is plainly

* Gregory, Lectures on "*The Eruptive Fevers.*" Am. Edition, 1851, p. 265.

evident that the matter obtained by Ceely was quite different from the animal virus of the Beaugency stock, now so well known in the United States.

It will be observed that writers upon the use of vaccine directly from the cow, especially as to the use of variolo-vaccine, give warning that a more violent form of vesicle would result from the insertion of such lymph; or, they instruct the vaccinator that frequent failures might be expected.

Dr. Seaton says (Handbook of Vaccination, p. 87): "Ceely found that more than half his attempts to vaccinate with primary cow-lymph, taken from vesicles at a proper stage, and possessing all the characteristics of perfection, resulted in entire failure, the same individuals being successfully vaccinated immediately afterward with the current humanized vaccine lymph."

With our knowledge of the course of a vaccination from the current Beaugency stock of animal virus, it is not difficult to believe that this virus which behaved so uncertainly had little relationship to the certain, normal, active stock now propagated in this country. All along through the history of the Ceely virus we read of uncertainties, of abnormal actions, and of suspicious phenomena, which virtually rendered its use impracticable, and which gave little encouragement for others to follow, except as a *dernier ressort.*

A new chapter on the production of artificial cow-pox was worked out during the late war, in the Confederate States. The question of the supply of genuine vaccine became such a serious one, that medical officers were commissioned to undertake the study of the immense number of spurious vaccinations, and devise the remedy. I speak within bounds when I say that in this country it was rare to find a medical man who was at all skeptical as to the possibility of procuring artificial vaccination at will. The experiments of Ceely were studied with peculiar interest, as were those of Sonderland. After attempting again and again to restore the efficacy of vaccine by careful cultivation among country children, no adequate supply was obtained, and then the variolation of cattle, by the Ceely and Sonderland method, was resorted to. Dr. James Bolton, late of Richmond, Va., undertook the experiments for the production of artificial

cow-pox. He says:* "I made experiments upon seven animals during the war. Five of them were young cows, or heifers, which were furnished me by the Commissary Department, by order of the Surgeon-General. They were kept at Howard's Grove, in the immediate vicinity of the Small-Pox Hospital. From the latter place I procured variolous virus in the form of lymph and crust. This I inserted into the udder by making light incisions, and rubbing into them while in the liquid state, by dipping threads into the lymph and passing them by means of a needle under the integument; and finally by making punctures with a lancet and introducing pieces of crusts; but all these attempts failed. In two instances pustules were produced, followed by thin yellow crusts, which were evidently spurious results." * * * "I have my doubts that genuine vaccine is obtained by inoculating cows with small-pox virus." Subsequently Dr. Bolton wrote me: "I have my doubts that the small-pox virus can be inserted at any part of the udder from which it cannot be licked off. The tongue of the cow can be protruded so far, and can be drawn into such anomalous curves, with such prehensile power, that I think any part of the udder can be reached.". The details of Dr. Bolton's experiments were destroyed by the casualties of war, and his account was written from memory.

Dr. S. P. Crawford, of Greenville, Tenn., writes to Prof. Joseph Jones, M.D.:† "About this time" (January, 1865) "orders were issued from Surgeon-General Moore to procure fresh vaccine virus through the cow. I tried the experiment three times, without succeeding. First on a cow seven years old, with a calf six months old; then a young heifer two years old, and finally the calf. I inserted the matter in the teats, nose, ears, and various parts of the skin, but never succeeded in getting a crust. Finally I fed the calf on the dried scabs, but it died without yielding the much sought treasure."

Dr. William J. Love, of Wilmington,‡ "had occasion to at-

* Non-Identity of Vaccinia and Variola, by Thomas F. Wood, M.D., 1867, p. 17.

† Researches Upon "Spurious Vaccination" During the American Civil War. By Joseph Jones, M.D. Nashville, 1867, p. 99.

‡ Non-Identity of Vaccinia and Variola, p. 17.

tempt the same experiment" [variolous inoculation of a cow], "and failed." He introduced the small-pox virus into the teats and udder, by means of a saturated thread. He also made incisions a little deeper than in the human subject for like purpose, and introduced the lymph in this way. Finally he smeared collard leaves with the lymph, and fed the cow on them. He saw no other phenomena, except such as would result from the insertion of any foreign matter,

I had occasion just after the war (1865–'66), while in charge of the Wilmington Small-Pox Hospital during an epidemic of the disease, to go over the same ground of attempting the production of artificial cow-pox. My experiments were done without any knowledge on my part of the details of the operation, except such as I had received through the gentlemen above quoted, and by the perusals of Ceely, Thielé's and Sonderland's experiments. It is not necessary to detail these experiments,, as they differed in no respect, as far as I was able to judge, from the description given by these authors. The failure was complete, in my opinion.

It happened, though, that during the progress of the experiment, that an army medical inspector, whose name I have forgotten, was making a tour of the hospitals, hearing of my experiments, visited my hospital, and after examination pronounced the small vesicles genuine cow-pox, and confirmed his faith in his opinion by making some inoculations on the arms of two children in an Irish family near by. The inoculation resulted in a genuine small-pox, which went through the family in various grades of intensity.

A reviewer in the *Medical Times and Gazette*, July 2, 1881, discussing Mr. Fleming's recent work, "Human and Animal Variolæ," says: "It seems to be commonly held among medical men in this country that cow-pox is not a disease natural to the bovine species, but merely small-pox conveyed to them from man by inoculation, and that in the operation of vaccination we avail ourselves of the modification which it has undergone in the body of the cow, precisely as Dr. Greenfield confers immunity from anthrax on cattle by inoculation with bacilli which have passed through the system of the rabbit.

"In support of this view, it is usual to adduce the supposed

rarity of cow-pox now that small-pox has become almost unknown in rural districts, the asserted exemption of the male sex among cattle, the successful inoculation of cattle with small-pox, with the production of what Thielé and Ceely believe to be cow-pox, and the vaccination of infants therefrom, by the same observers.

"Mr. Fleming has collected a vast amount of evidence from French, German, Scandinavian and Italian sources, and has carefully scrutinized the scanty English experimenters. He gives irresistible evidence of the prevalence of cow-pox in countries where small-pox is almost unknown,* of the equal susceptibility of bulls, if exposed to the contagion with cows, and satisfactorily proves that the supposed vaccinations of Mr. Ceely were, in fact, nothing more or less than an inoculation with small-pox unchanged by its transmission through the cow. Such inoculation of the cow is not often successful, but in several instances the real nature of the lymph thence taken and implanted in children has been manifested not only by the eruption following, but by the communication of unmistakable and fatal small-pox to other infants by ordinary infection—a caution to those who would attempt to reproduce a more active vaccine through such a procedure as mediate variolation. * * * The disastrous results of mediate variolation instead of vaccination, in America and India, show the value of a knowledge of the diseases of domestic animals."

The *opinions* as to the non-identity of vaccinia and variola are very numerous, and writers are about equally divided, perhaps. It possibly may not be definitively settled in our day, and indeed, the question of identity may be no longer a practical one. This will depend greatly upon the way in which the medical profession makes use of the triumphant solution of the question as established in the introduction of animal virus by Dr. Henry A. Martin. It does not seem possible that with our present enlightenment, a vaccine famine can occur in this country, and for years no more attempts will be made to produce artificial cow-pox. Never-

* Cow-pox has appeared in communities where there has been no small-pox at the time. A recent most convincing series of cases were reported by Dr. Henry A. Martin, of Boston, at the last meeting of the American Medical Association. These cases of spontaneous cow-pox occurred in 1880, in Massachusetts, and their relation was rather to horse-pox than small-pox, a case of the former having also occurred in the same neighborhood.

theless, the warning should go forth authoritatively, that the production of artificial vaccination is a myth, and in the times of great danger from variolous epidemic, the time when it is most eagerly desired to do something effectual, it has most signally failed.

Let the decision be what it may as to the theory of identity, it is practically valueless, and always dangerous, both on account of the delay, and of the risk of pure variolous inoculation. Those who have claimed success have left such a record of difficulties to overcome, and disappointments to suffer, that we can no longer have faith in it as a source of vaccine, even though we should not discredit the truth of identity.

I had intended to discuss more fully the points in Dr. Lyman's paper on vaccination, above referred to, but I have overstepped the bounds in consideration, mainly, of the question of identity. I am pleased to see that we agree as to efficacy of animal virus. I have nothing but praise for the results of vaccination done with Beaugency virus. I trust that the potency of this virus may forever prevent the adoption of Dr. Lyman's suggestion: "The only efficient remedy for this deterioriation [referring to the possibility of the degeneracy of bovine virus] consists in occasional reversions to the original product of the variolated cow." I am convinced that more diligent searching by experts will detect now and again genuine cow-pox. From this source alone will the pure succession be maintained. Knowledge is accumulating very greatly upon the natural history of cow-pox, and a widespread interest will surely manifest itself among intelligent farmers, making the mishap of the non-occurrence of cow-pox less and less probable.

ARTICLE IV.

ECLAMPSIA, OR PUERPERAL CONVULSIONS. By D. A. WALDEN, M.D., Beatrice, Neb.

In May, 1879, I was called to see Mrs. H., a primipara, age 23 years, in her seventh month of gestation. Found her in puerperal convulsions, which would last from one to five minutes, and each paroxysm was followed almost immediately by another.

I remained with the patient all night, and administered chloroform by inhalation, but could not control the convulsions. In the morning I began to administer morphia sulph. in one gr. doses, but with no better results. I followed this with hydrate chloral, 60 grains every two hours, and bromide potass., 80 grains every three hours, and continued this treatment for forty-eight hours, without controlling the convulsions in the least.

Counsel was called, and a delivery was concluded to be the only means of saving life. Fœtus was dead—woman made perfect recovery.

In March of the year 1881 the same woman was taken, in her eighth month of pregnancy, with puerperal convulsions the second time. I was called to see her, and found her almost as before; gave her the same remedial treatment as before, but without any good results.

The idea occurred to me: would not quinia have good effect in such cases? I concluded to try it, at least, as I had tried all remedies that I could find in books on that disease. I wrote a prescription for quinia sulph., grs. 80, to be divided into three powders, one to be given every two hours.

After an hour from the time the first powder was taken I could see a change; the convulsions were becoming lighter. Continued the quinia until six 10 gr. powders were taken, at which time the convulsions were entirely controlled, and did not again return.

The patient went to her full term, and then I delivered her of a healthy female child. The mother and child are doing well up to this date.

Case second. Mrs. B., a multipara, age 24 years, in her eighth month of pregnancy, was taken with puerperal convulsions on July 8, 1881. I was called to see her; found her in convulsions most of the time; remained with her all night, and gave chloroform by inhalation without any effect. Then remembering my former case, I ordered quinia sulph., grs. 60, divided into six powders, one to be taken every two hours. A change was noticed in about an hour after the first dose was taken, and the convulsions had ceased entirely by the time the six doses were taken, but came on again the next day. I then ordered the quinine again, and continued it for three days.

The convulsions did not again appear, the patient went to her full term, and I then delivered her of a healthy male child.

Case third did not come directly under my notice, yet I will cite it, to further prove that there is really some virtue in quinia for this disease.

After trying quinia in my first case, I related my experience to Dr. S. G. Huff, of this place, and soon after, he, having a case of the same kind, thought, after trying the usual remedies of bromide potass., chloral hydrate, chloroform, etc., that he would give the quinia treatment a trial, as the more that bromide potass., chloral hydrate, etc., were given, the worse the patient became. The convulsions were so severe that the assistance of two or three persons was required to keep the patient in bed.

He put her on 10 gr. doses of quinia, one to be given every two hours; it controlled the convulsions completely, and she has not had a return of them since. Her entire term of gestation is now nearly up.

I will not enter into the pathological condition of puerperal convulsions, for that has been fully discussed by all of our eminent authors, yet I would say to the profession that it may be of some interest and benefit to give quinine a fair trial in this disease.

ARTICLE V.

STUDIES IN THE MEDICAL BOTANY OF SOUTHERN ILLINOIS.

By J. M. G. CARTER, A.M., M.D.

The study of botany is exceedingly interesting, not only on account of the pleasure derived from considering the endless varieties of plant, flower and fruit, but on account of the healthful exercise and growth of mind connected with its observations, investigations and classifications. The study becomes doubly interesting when in addition to the foregoing we take into consideration, as the physician does, the remedial value of the active principles of plants in disease. Whatever is life-saving or health-giving has a peculiar charm about it, and when we consider that the vegetable kingdom furnishes us most of these desirable agents, we have probably the strongest incentive to its study which could

draw the mind to the consideration and investigation of any science known to man. Every physician ought to be acquainted with the science of botany so far as it pertains to medicine, and especially the medical botany of his own neighborhood. Not unfrequently he is called to a case (particularly is this so in country practice), where a medicine is required which he does not happen to have with him, but which may be growing in the woods near by. An acquaintance with this branch of medicine at such a time as this can easily be appreciated.

I do not pretend to say that there is a relation between the diseases of a country and its flora, nor venture an opinion as to what that relation, if there is one, may be; but I believe it is an established fact, that where a particular disease seems to be indigenous, *there* is found in the vegetable kingdom a remedy for its cure, and very frequently the best remedy. In this place I might refer to malaria in all its forms, so tenacious in this part of the State, to relieve which we have wahoo, dogwood, mandrake, etc. The same disease prevails in South America, and no remedy has a wider reputation than cinchona, of the same country.

A large proportion of all the medicinal plants indigenous to the United States can be found in Southern Illinois; but those that are used especially in the treatment of diseases of this climate are most abundant.

In this paper I can only refer to the families of plants and some of the more important individuals; those not supposed to be well known, and those having some interesting remedial application in domestic practice among the people of this portion of the State. For descriptions of these plants, I must refer the reader to Gray's Manual, or any other good manual of the botany of Illinois.

The crowfoot family (Ranunculaceæ), the order to which aconite and coptis belong, is represented in Southern Illinois by a score or two of medicinal plants, the most important of which are Hydrastis Canadensis and Cimicifuga Racemosa. Yellow puccoon root is frequently used in decoction or infusion for "chills," jaundice and debility, with excellent effect. It is also used with benefit as a mouth wash in various forms of stomatitis. I use the powdered root with chlorate of potassa or alum, in

ulcerated sore mouth, and I have often found the combination to be superior to the potash alone. The people not uncommonly use an infusion of the root as a wash for ulcers. Cohosh, or blacksnake root, is often used in rheumatism, dropsy and fevers.

The magnolia family (Magnoliaceæ) has but one representative so far as I know. This is the Liriodendron Tulipifera. It is a magnificent tree, generally called poplar. The bark of any part, especially the root, may be used in infusion or powder as a tonic, and in malarial fevers, more particularly intermittent. The ordinary strength of the infusion is ℥j to Oj of water. The powder may be given in doses of 30 to 60 grains.

Podophyllum Peltatum is the only plant of the barberry family (Berberidaceæ) which deserves mention in this paper. This plant, known as the May apple, or mandrake, is largely used in this portion of the State in bilious fevers (remittent and intermittent); it is sometimes combined with calomel and sometimes given alone, or in combination with other vegetable cathartics. In these fevers, when it is necessary to keep up cathartic action for several days, I frequently give two or three full doses of calomel, and follow with podophyllum and blue mass. In splenic dropsy mandrake in connection with tonic doses of quinine is a most valuable remedy.

Poppy family (Papaveraceæ). I am not aware of any indigenous variety of the poppy in Southern Illinois, but the Papaver Somniferum is occasionally cultivated and used in domestic practice, in infusion or decoction, as an anodyne to sores, and sometimes internally. It does not seem to be as strong as the eastern or continental plant; perhaps because not so much care is bestowed upon its cultivation here as in the old world. I know of no experiments with the poppy produced here to determine its value; but presume its value is equivalent to that of any other portion of the United States. Opium, in its various forms, is very abundantly employed in both general and domestic practice, in the malarial diseases of this section. However, there is quite a difference of opinion in regard to the relative value of the various preparations. Some use the pulverized opium, many Dover's powders, and many more morphine. The great drawback to the employment of any of these through the stomach, is their consti-

pating effect, as it is considered by nearly all physicians in this country that the bowels should be opened immediately in bilious fevers. To avoid the constipating tendency of opium, some prescribe svapnia, others use morphine hypodermically. My experience with the hypodermic injection of morphia has not been extensive enough to make me feel safe in making a positive statement that so used it does not constipate; still I have thought that I could see that it was not likely to do so, and many physicians are very confident in their assertions that used in this way, morphia does not affect the bowels.

Sanguinaria Canadensis, another plant of the poppy family, is very abundant in this part of the State, being known by the name of *red puccoon root*. Blood-root is used in general and domestic practice as an alterative and tonic chiefly, in amenorrhœa and inactive liver, in cases of chronic malaria, etc. It may be used in combination with Podophyllum Peltatum in splenic dropsy. It is used by the laity principally as "bitters," the dried root in some form of spirituous liquor—usually whisky. Occasionally it is so used in combination with Hydrastis Canadensis. It must be used in small doses, and continued for considerable time. I had a case of acute jaundice last January, due to obstruction of the gall duct—an effect of malaria. The case became chronic, and I used sanguinaria for two months, with ultimate recovery.

The mustard family (Cruciferæ) is another order with a large representation in our section, but I shall mention only three, the others not being of any great importance. Nasturtium Armoracia (horse-radish), Sinapis Nigra (black mustard), and Sinapis Alba (white mustard), are very important plants, and the first two especially are used in domestic practice as emetics and counter-irritants. Not unfrequently, when I have not been able to secure mustard, I have had horse-radish scraped, and used as a poultice to produce counter-irritation in bilious pneumonia, or "winter fever." The results have been good, and I have no doubt that it might be safely used as a substitute for mustard in any case where mustard is indicated, provided care be taken. The mustards (black and white) do not seem to be indigenous, but were first brought from Europe, and have now escaped from

gardens. Horse-radish is native in this State. Treacle mustard (Erysimum), and hedge mustard (Sisymbrium) may be used as substitutes.

Besides prickly-ash (Zanthoxylum), the rue family (Rutaceæ) contains wafer-ash, or hop-tree (Ptelea Trifoliata). Wafer-ash is an excellent tonic to the stomach and intestines, especially in chronic malarial and typhoid cases. Combined with pepsin, it is greatly beneficial in dyspepsia. I have used it with good results, thus combined, in cases where sometimes "heartburn" and sometimes distressing pain is felt after eating. I have often given it four or five hours after a meal, when the stomach is disturbed and painful, and have seldom known it to fail to give immediate relief. An excellent preparation is manufactured by Chapman & Green, of Chicago.

The remedy may be used in infusion, or better, in tincture prepared, according to Dr. Potter, by the following formula:

℞. Hoptree bark.....℥ vi.
Ginger.....℥ ss.
Whisky.....O iv.
M. Sig. Tablespoonful thrice daily.

The Cashew family (Anacardiaceæ) also contains some plants of considerable remedial value. In a country where so much calomel is used as in this, and where at least half the people have been salivated, it is of great service to have a remedy specially adapted to cure such cases of sore mouth. Sumach (Rhus Glabrum) has a high reputation in this malady. It has also been recommended as a cooling drink in febrile complaints. An infusion of the berries is used. It is somewhat astringent.

Poison ivy, or poison oak (Rhus Toxicodendron), concerning which so much has been said and written within the past few years, belongs to this family, and is very abundant in Southern Illinois. The poison vine (R. Radicans) is also abundant. The last two plants, and some others of the same family, are not used, so far as I know, in domestic practice, and are only feared by the people for their poisonous properties.

The principal representative of the staff-tree family is wahoo (Euonymus), or burning-bush, sometimes called Indian Arrow. The variety I speak of is Euonymus Atropurpureus. I have already mentioned this as an important remedy in malaria.

This plant, or shrub (Nat. Order Celastraceæ), is found in low grounds and swamps. The bark is the part used, an infusion of which is often taken as a tonic in chills and fever. It has been used with success in dropsies. In its cathartic action it somewhat resembles rhubarb. It is a diuretic, and supposed to be an alterative, acting especially on the liver. It is used in infusion in splenic dropsy.

Ths large rose family (Rosaceæ) is well represented in our woods. The wild cherry (Prunus Virginiana, P. Scrotina) belongs to this family, and is abundantly used as a tonic with iron, quinine and wahoo. Its tonic value is seen in general debility, malaria, and hectic fever of consumption. To this family belong also strawberry (Fragaria Illinoiensis), raspberry (Rubus Odoratus, R. Strigosus, R. Occidentalis), blackberry (Rubus Villosus), and dewberry (Rubus Canadensis). The root of all these may be used similarly to the blackberry of the U. S. Dispensatory.

The saxifrage family (Saxifragaceæ), in addition to several other medicinal plants, is represented in alum-root (Heuchera Americana). This root is used where other astringents would be indicated, and at one time had quite a reputation as a cancer medicine. It may be used in powder, decoction or infusion.

Passing by Hamamelis and Liquidambar, of witch-hazel family, Œnothera, of primrose family, all of which are valuable remedial agents, I mention the parsley family (Umbelliferæ), chiefly because it contains Conium Maculatum, a most excellent substitute for opium when it is desired to avoid constipation. This plant is said to grow in Southern Illinois; however, I have not seen it. Deucus Carota, common carrot, belongs to the same family. An infusion of the root is an excellent nephritic remedy in chronic cases. It is used in dropsy. Water hemlock (Cicuta), parsnip (Pastinacea), and other indigenous medicinal plants belong to the same family.

To the ginseng family (Araliaceæ) belong false sarsaparilla (Aralia Nudicaulis) and ginseng (Panax Quinquefolium). The root of the latter is used in whisky, by some people in this part of the State, as a general tonic. This plant is valued very highly by the Chinese, as a remedy in all diseases.

The dogwood family (Cornaceæ) is another family useful in medicine. Dogwood (Cornus Florida) is an excellent substitute for quinine. It is not unfrequently used as a tonic, in the form of a tea or bitters made of the bark. When quinine has failed in intermittents, dogwood often will effect a cure. Other varieties of dogwood (C. Cincinata, C. Sericea, C. Asperifolia), may be used as substitutes for Cornus Florida.

Only fifteen of the thirty-one orders, or families, of polypetalous exogenous plants, found in Southern Illinois, that contain medicinal plants, have been mentioned; and these thirty-one orders do not contain forty per cent. of the medicinal plants indigenous to this portion of the State. The fifteen orders mentioned contain the most important remedies of this botanical division. The remaining botanical divisions are left for future study.

This outline of the medicinal plants in only fifteen families is sufficient to show that our State is rich in medicinal agents. One hundred plants might have been given, instead of less than forty; but perhaps the list has been sufficiently extended.

ARTICLE IV.

THE PATHOLOGY AND TREATMENT OF YELLOW FEVER; WITH SOME REMARKS UPON THE NATURE OF ITS CAUSE AND ITS PREVENTION.

By H. D. Schmidt, M.D., New Orleans, La.

(Continued from page 312, September No., 1881.)

In drawing a final conclusion from what has been said in the preceding pages upon the bacteria question, we may well presume that they do not represent the direct cause of those diseases with which they have been found associated, unless stronger and more reliable proofs are brought forward in favor of this theory. But, as by this time, in most diseases, the blood and tissues have been examined over and over again by numerous observers, who are anxious, besides, to "find something," without the discovery of other facts than those known for a number of years, it appears quite improbable that this will ever occur.

In order to show that all statements, regarding the presence of minute organisms in the blood and tissues of individuals affected

by infectious diseases, must be received with great caution, I shall, before dismissing the theory of the "contagium vivum" in general, cite the views of a few of the most pronounced germ-theorists on the subject. Thus, *Liebermeister*, who theorizes even upon invisible germs, says, more than six years ago:* "It is true that the triumph of the theory of a contagium vivum is in no way complete, and as in former times, so now, it is not so much its opponents, as it is the imprudent adherents who threaten to bring the theory into discredit. The utter lack of critical discernment and method which have characterized some of the works in this field, and, on the other hand, the recklessness with which facts of uncertain significance have been proclaimed certain proofs, have also in our time driven away many earnest investigators."

Koch,† the discoverer of the spores of Bacillus anthracis, after testifying that "in the blood and tissues of the healthy animal or human organism bacteria are not met with," continues as follows: "It appears to me, however, that the following objections against the acceptation of bacteria being the cause of infectious traumatic diseases are justified. To gain an incontestable proof for this acceptation it is essential *that the bacteria, without exception and in proportions regarding their number and distribution, should be shown in such a manner, as to perfectly explain the symptoms of the respective disease.* For, if in some cases of a certain kind of infectious traumatic disease bacteria are found, while they are absent in other cases exactly alike,—and if, furthermore, the bacteria are present in too small a number as possibly to cause a severe disease or even a fatal issue, it becomes self-evident that their inconstant appearance is dependent upon chance, and their small number no sufficient proof for regarding them as the sole cause of the respective disease, and that we must accept the existence of other causes besides. The observations made in regard to the presence of bacteria in infectious traumatic diseases, however, do in fact not correspond with the demands required for an incontestable proof."

* Cyclopædia of the Practice of Medicine, edited by Dr. H. v. Ziemssen; American edition, Vol. I.

† Koch, R.—Untersuchungen ueber die Ætiologie der Wundinfections-krankheiten. Leipzig, 1878, p. 22.

Birch-Hirschfeld,* in speaking of the pathological signification of bacteria makes the following remarks: "But it must be admitted that, even if the constant occurrence of these organisms in the diseases mentioned is recognized as a fact, doubts may yet arise as to whether these bacteria are the real cause of the disease."

"This question will not arise with an infectious disease in which organisms of a certain morphological character constantly and exclusively occur, as in recurrent fever, where the spirilla claim a diagnostic signification. In splenic fever this circumstance is less significant."

"The morphological character of the bacteria found in pyæmia, diphtheritis, small-pox, cholera, however, are so much the same, as to give rise to the idea that they are identical forms. But the conclusion drawn from this would be that no specific signification could be assigned to these organisms. Accordingly, they would not represent the cause but parasites of the disease, which, even then, may not be without signification, or may stand in some relationship with certain septic conditions of some infectious diseases."

After the preceding discussion of the so-called "bacteria theory" and of those diseases in which these minute organisms are really met with in the blood and tissues of many cases, we are now ready to inquire *whether this theory may in any way be applicable to yellow fever;* and, in order to arrive at correct conclusions from such an inquiry, it becomes essential that we should set aside all postulation, such as the existence of "invisible" living germs, and strictly keep to observed positive or negative facts. It is self-evident, therefore, that the assertion of bacteria or their spores being the cause of yellow fever must be substantiated by the ocular demonstrations of these organisms in the fresh blood of the patient, before it can be received as a truth. Judging from my own numerous examinations of yellow fever blood under various conditions, already mentioned in this treatise, I need not hesitate to say that this has been done. And, since the results which I repeatedly obtained ever since the yellow fever epidemic of 1867, have now been corroborated by those

* Birch-Hirschfeld.—Lehrbuch der Pathologischen Anatomie, p. 233.

of *Sternberg*, who, during 1879, examined quite a number of specimens of fresh yellow fever blood at Havana, I may add that all statements to the contrary must have been founded upon an imperfect and inexact manner of microscopical examination. And as, furthermore, bacteria are neither met with in the tissues and organs of fatal cases of yellow fever after death, if properly prepared while still in a fresh condition, we are justified in presuming without further extending the argument, *that yellow fever does not depend upon the presence of bacteria or any other minute living organisms in the blood or tissues*, for, if it did, *the microscope would most certainly show them*, as well as it has done in those other particular diseases mentioned.

But, it will now be asked, if the cause of yellow fever is not represented by minute living organisms, what is it then? And there are many persons, both physicians and laymen, who will promptly answer this question by referring the cause to the products of vegetable and animal decomposition in the form of filth and dirt in the streets and houses. But before I shall attempt to show that the "filth-theory" has no better foundation than the "living-germ-theory," it becomes necessary that I should properly define the standpoint from which I view the subject, as otherwise I might be looked upon as an advocate of dirt and filth. There is hardly a physician who does not consider a supply of fresh and pure air and water as the cardinal objects in public and private hygiene; nor have I, myself, ever regarded these elements in any other light. On the contrary, the degree of civilization of a community may rightly be judged from the degree of neatness and cleanliness of the houses and streets; and nobody will doubt but that the hygienic condition of a community depends to a considerable extent upon the purity of the air breathed by the people in their dwelling-places. It is also true that some local affections may arise from the breathing of air contaminated by gases arising from privies, or from other animal and vegetable decomposition. Such cases may occur in any large city, though not very often; but, that a *specific* and *contagious* disease, characterized by *specific symptoms and pathological changes in the tissues and organs*, should take its origin from such a cause is contrary to all modern scientific knowledge. A

specific disease can only be caused by a specific poison; and though certain pathological processes may equally take place in different specific diseases, yet each of the latter maintains its own specific character besides. The poison of yellow fever will never produce scarlatina, and the poison of the latter will never give rise to yellow fever, though in both diseases parenchymatous degeneration of the kidneys may be met with. The development of a specific disease may be compared with that of the egg; for a certain mysterious agency—similar to that determining the specific characters of an animal or plant, and lying dormant in the protoplasm of the egg-cell—also resides in the poison of a specific disease, determining the specificity of its symptoms and pathological changes. And, accordingly, the inquiry into the origin of these specific poisons will be as resultless as that into the origin of the first egg. As far as I am able to judge, it appears to me that specific diseases have never sprung suddenly into existence, but that their origin must be compared to that of vegetable and animal species, and that, throughout an immense length of time, they have been developed into what they are now by the processes of "selection" and "adaptation." Thus, small-pox, or yellow fever, may have presented a different character thousands of years ago, from what they do at present.

Yellow fever, therefore, being a specific disease, cannot arise spontaneously through the influence of the air rendered impure by the products of vegetable and animal decomposition, for, if this were true, New Orleans would be depopulated long ago. From all I know, the disease, in this city, has never arisen at the outskirts, or in the vicinity of those places to which the kitchen refuse and rubbish of the houses used to be carried not many years ago; or of grave-yards, such as the Locust Grove Cemetery of bad repute, but, on the contrary, always in the populated commercial centers, or in the vicinity of the harbor. Besides, it is a historical fact that this disease almost always first appears in a sea-port, before it extends into the interior of the country, and that it stands in close relationship with the shipping over sea. If yellow fever could ever have originated from the gases arising from decomposing matter, it should have certainly done so in the vicinity of one of those so-called dumping grounds, a large empty

lot, situated, not many years ago, on Broad Street Canal between St. Anne and Dumaine Street, and which, in the pursuit of my practice, I had frequent occasion to pass. Whenever I passed, I found it occupied by quite a number of negro rag-pickers of both sexes, accompanied by their dogs, and engaged in digging the ground in search of lost treasures. The stench arising from this place was so offensive and sickening in character that I had to rouse my horse and hold my breath until I had passed the whole square. In the same way I have frequently met with offensive odors, though differing in character from that of the garbage, arising from the stagnant water of the canal itself, yet I never heard of yellow fever originating in that vicinity, nor did I learn that other diseases prevailed to a greater extent there than elsewhere. As regards the spontaneous origin of diseases in the vicinity of the grave-yards in the city, I need only point to the report of a committee, appointed by the Medical and Surgical Association of New Orleans, for the investigation of this matter, and which answered the question in the negative. Many other instances might be adduced to show that no relation exists between the origin of yellow fever and the filthy condition of some of the streets of New Orleans, or any other city. With this assertion, however, I by no means advocate the existence of grave-yards in the heart of the city, or bad drainage and muddy and filthy streets and houses, for there remains no doubt but that whenever the the disease has once been introduced and assumed the character of an epidemic, nothing will contribute more to it being fostered and propagated than filth and insufficient ventilation. This I know from experience.

It has also been asserted that yellow fever was caused by gases arising from the bilge-water in the hold of the ships, and the latter has even been regarded by some germ theorists as the medium by which the "contagium vivum" was transmitted from port to port. As regards the gases emitted from foul bilge-water, the remarks made on filth and dirt will be equally applicable to this liquid; but, as concerns the living germs which it is supposed to contain, the supporters of such a theory seem to have forgotten that these minute organisms are all inhabitants of the water or other liquids, which they are incapable of leaving except

by evaporation; for, minute as they are, they are still too heavy to be lifted out of the liquid with the molecules constituting its vapors, while in a dry condition they may be carried about by the slightest current of air.

Besides the reasons I have given above for showing the futility of the theory of yellow fever originating in filth, or from the gaseous products of decomposing matter, I might still further enlarge upon this subject,—a subject which, by having been treated so often and so extensively, both in medical and public literature, is now too well known to require any further comment on my part. Nevertheless, in support of my own views, I cannot forbear to cite the views of two distinguished germ-theorists before dismissing it. Thus, Liebermeister* says: "Not a very long time ago it was almost universally accepted that merely the coincidence of certain especial conditions were necessary to cause the autochthonous appearance of certain infectious diseases. As early as the time of the Athenian plague, Diodorus found a sufficient explanation for the origin of the disease in the circumstance that a great multitude of people from all quarters streamed into the city, and, being cramped for room, breathed a corrupted air; "thus they were attacked with disease in an inexplicable manner." How many thousand times since then have medical and non-medical writers pictured social squalor, decomposing filth, unfavorable weather, etc., as the cause of disease! And physicians and laymen were accustomed to take it for granted that the plague and similar severe epidemics originated after this fashion. Many physicians found no difficulty in explaining the origin of syphilis from the mingling and concentration of the immoral elements of numerous nations. The plague arose from the imperfect burial of human corpses, with the consequent corruption of the air; yellow fever originated from foul bilge-water or from crowding of men together in slave ships; typhus fever from the crowding together of persons in badly ventilated dwellings, or from hunger; the cholera from rotten or unripe vegetable food; the typhoid fever from the exhalations of putrid excrements, etc. For many it was an interesting spectacle to realize

* *v. Ziemssen*, "Cyclopædia of Practice of Medicine," Vol. I, p. 17.

how the great regulatory operations of nature should be so simple and easily understood, and how every considerable deviation from a proper observation of hygienic laws should immediately be punished by the production of a particular disease. In recent times the standpoint has been essentially changed. The potency of the factors in the *extension* of the diseases in question is not questioned; on the contrary, our knowledge of it has become more reliable and exact. We have learned, however, that the diseases do not *originate* in this way. It was observed that the battle-fields of Inkermann, strewed with corpses, whose stench drove the armies away, produced no pestilence. We have been convinced that, in spite of the prophecies and premature results, the investment of Metz was never able to produce a single case of typhus fever, inside or outside of the city, or to transform typhoid fever, which prevails there so frequently, to a possibly higher potency, *i. e.*, to typhus fever. We have gradually reached the conclusion that it is only where the specific germ of the disease exists by itself, or has been introduced, that those anti-hygienic factors become active and may then be capable of occasioning an enormous extension of the disease. The germ, however, is not produced by spontaneous generation."

"In fact, at the present time the acceptance of an autochthonous origin for syphilis is, for most physicians, as great an absurdity even as the origin of scabies or of lumbricoid worms by spontaneous generation. It is unanimously said of the plague, cholera, and yellow fever, that they never arise spontaneously, at least on European soil. The spontaneous origin of small-pox, measles and scarlet fever could hardly find a defender now. Perhaps the time is not far distant, when the doctrine of the spontaneous origin of typhoid fever, dysentery, typhus fever, etc., will be universally rejected."

Naegeli, from whose work on "The Lower Fungi and Their Relation to Infectious Diseases," I have already quoted in this section, speaks of the influence of the air upon these diseases as follows: "The air, containing bad smelling or other gases, though disagreeable, is not rendered noxious by it, particularly with regard to infectious diseases. With it, however, the different forms of more or less dangerous cleft-fungi, suspended in the dust-like masses, may be introduced into the body."

"The infected air itself is odorless. A putrefying substance only becomes dangerous after it has become dry, and the bad odor has disappeared. The miasms and contagia are also imperceptible for the organ of smell."

"The microscopical and chemical examination of the residue of filtered air teaches us nothing as regards its noxiousness, as the infectious matters cannot be distinguished. It will be impossible to pass a judgment upon the infectious nature of any air, before we are able to test the effects of this residue in an experimental manner."

"It is impossible to render infected air innoxious, as it cannot be made free from dust. At the utmost, the dust suspended in the air of an inclosed space may be removed by the vapor of water, or by sprinkling the walls."

By some the origin of yellow fever, as well as that of other diseases, has even been referred to our drinking water becoming pregnant with decomposing vegetable matter while standing in the over-ground cisterns. I shall once more draw from Naegeli's work and cite this author's opinion on this subject. He says: "The drinking water (from wells, rivers, ponds, lakes, ground-water), even if not clean, is not deleterious to health (provided poisons do not get accidentally into it), and, least of all, causes infectious diseases, as those substances rendering it unclean are generally harmless in their nature; the fungi of putrefaction and putrid substances, as well as miasma-fungi, which it contains, represent so small a quantity, as to reduce the probability of a contagion to an imperceptible minimum."

"With this corresponds the experience, which shows that the continual eating of large quantities of humus substances and products of putrefaction, as are contained in foul drinking water, as well as the exclusive and constant use of the latter by whole populations, are without deleterious hygienic effects. A critical consideration of the known facts relating to the spread of infectious diseases, also, shows that contagion has been wrongly ascribed to the water."

The idea that the air we breathe is pregnant with fungus spores and bacteria, and that hundreds of these minute organisms are inhaled with every inspiration, seems to prevail among a

number of medical men; and it is this coarse error which has rendered the germ-theory so acceptable to the medical practitioner. It is true that under certain conditions these minute organisms are met with in the air, though in a comparatively small number, for the reason that the natural media in which they live are fluids in a certain state of decomposition, in which they multiply to countless numbers. Their comparative scarcity in the air has been satisfactorily proved by the observations of *Cunningham*, made some years ago at Calcutta, for the purpose of determining whether there existed any definite relation between the quantity of organic matter in the air and the prevailing diseases, such as diarrhœa, dysentery, cholera, miasmatic fever and dengue, or whether these diseases depended upon certain forms of organisms suspended in the surrounding air. For this purpose he used a weather-vane in the form of a tube, containing a plate of glass covered with a layer of glycerine, to which all particles contained in the surrounding air would adhere. The apparatus was placed in the vicinity of a prison and five feet above the ground. The matter adhering to the glycerine consisted of particles of silica, amorphous granules, carbon, lime, starch granules, cells, hair, fragments of vegetable tissue, filaments of cotton, hairs of insects, oil globules, pollen granules, spores and cells of fungi and algae, sometimes also bacteria, but in a small number. It was impossible to show a relationship existing between the amount of these particles and the diseases above mentioned.

In New Orleans, where rain-water, collected from the roofs of the houses into over-ground cisterns, is used for drinking or cooking purposes, similar objects are frequently found in the water, especially after a long draught, when the dust floating in the air has had sufficient time to settle upon the roofs of the houses. But I have never heard that diseases have arisen from the drinking of the cistern-water containing these miscellaneous particles, except, perhaps, when the water had been standing for a long time in an old foul cistern,—and even then such an occurrence would be very improbable. On the contrary, our drinking water is the purest that could be anywhere obtained, especially after being filtered, as it is in reality distilled water, in the place of which it is also generally used by the apothecaries. I have been

using this water now for many years in my microscopical examinations, but I do not remember ever to have met with bacteria in it, though, if used unfiltered, minute fragments of vegetable tissue or other particles may be met with. Bacteria do not appear until putrefaction has set in.

A number of times I have had to listen to the remarks of medical friends that living germs might exist in the air, too minute to be seen by the aid of the microscope. To this I can only reply that the modern "germ-theory" has not been based upon "invisible" germs. Even Hallier and Pasteur, the most persevering germ-theorists, never attempted to substantiate their assertions by things they could not see. They only failed in proving that the germs or organisms which they did see represented the true cause of disease. The smallest organisms known at present are the monads, bacteria and vibrios, representing minute granules, either single or arranged in rows, as already mentioned. But minute as they are, they really require no high amplification to be perceived in the form of a dot; it is only for the study of their details that high magnifying glasses and correct illumination are required. And, as stated before, the modes in which they multiply have even been studied by the aid of the microscope. The modern microscope is fully able to show things much smaller than these organisms, regarded by the germ-theorists as the cause of the disease; and, unstable as the germ-theory may be, it is surely not based upon imaginary objects. Its failure only consists in its inapplicability to the phenomena associated with the pathological processes of the disease in question.

In the preceding pages I have endeavored to demonstrate that the cause of yellow fever cannot be assigned to the noxious influence of bacteria in the air, for the potent reason *that they are not met with in the blood and tissues* of yellow fever patients, either before or after death. Neither can it be assigned to the noxious effects of the breathing of air contaminated by the products of vegetable or animal decomposition, or to foul drinking water, for such a supposition would be, as I have endeavored to show, in opposition to all experience of modern science, teaching that infectious diseases never arise from these last mentioned

causes. There is, however, another "invisible" agent left, to the noxious influence and effects of which all the characteristic features of the disease may be referred. This agent represents a certain specific noxious poison, *being a product of the diseased organism itself*, and, in consequence, *of animal origin*. To show the probabilities of this supposition, and to explain the more than probable manner in which this poison is reproduced and increased in quantity, as well as transmitted from individual to individual, will be our next task.

When, in 1867, the yellow fever appeared again at New Orleans, and soon extended over the city in the form of an epidemic, I adopted the view then generally entertained by the larger portion of the medical profession and the people of this city, that the disease was non-contagious; and as the fungi-theory, originating in the statements of Klob and Hallier, was then prevailing among medical men, I also inclined to the view that yellow fever depended upon some kind of micrococcus floating in the air. And, in accordance with this idea, I examined the mucous membrane of the alimentary canal and respiratory passages in quite a number of cases at the Charity Hospital, but without discovering any micrococci or other minute organisms in those localities; in the same way I failed to discover anything abnormal in several specimens of blood from hæmorrhages of the nose occurring in some of my private patients, or from specimens taken from the dead body at the Charity Hospital. It was only in the black vomit, as already mentioned, that I met with fungi and their spores. But as it was obvious that the organisms contained in this fluid could not have any relationship with the primary cause of the disease, I did not hesitate to discard the fungi-theory, while retaining the view of the non-contagiousness of the disease. And, in view of the analogy certainly existing between some of the symptoms of miasmatic and yellow fever, I became inclined to regard the latter as a miasmatic disease in the most intense degree, depending upon a poison arising from a combination of decomposing vegetable and animal matters. A reason for this view was that the disease principally prevailed in cities in which large numbers of men and animals lived in close proximity along the borders of swampy rivers. This view, which is

still entertained by a number of physicians, I favored until the yellow fever epidemic of 1878 made its appearance.

Previous to this time my studies had been principally directed to the pathology and treatment of the disease; and, I must confess that, since I failed to demonstrate the primary cause of yellow fever upon the theory of a *contagium vivum*, I had paid but little attention to this subject afterward. But when I observed, during this last epidemic, the disease slowly extending, frequently from house to house—and, in many instances, after having once entered a house or a family, not leaving it until it had attacked every inmate who had not been affected by it before,—when I saw it traveling from one district of the city to another, new centers of infection arising in places to which the poison had evidently been carried; and when the epidemic slowly extended from our city to neighboring communities—especially along the railways and other routes of public travel,—even to villages and plantations in the country, my attention was again directed to its cause, and by subsequent and more systematic studies and observations I became convinced of its contagious nature.

It would be impracticable to enlarge upon this subject in this place by citing and discussing individually the numerous cases and instances which speak in favor of the contagiousness of this disease, I therefore only refer the reader to the innumerable reports, both of ancient and recent date, containing the history and statistics of yellow fever. I may, however, remark that any physician who will take the trouble and devote the time to an impartial and philosophical study of the subject in all its details, following the outlines which I have sketched in this treatise, will finally become convinced, like myself, of the contagious nature of this disease. My own observations and studies have even inclined me to regard the infectious poison of the latter as more subtile and virulent in its nature than that producing small-pox.

Yellow fever, then, is a disease, running a regular course, and accordingly depending, like small-pox, scarlet fever, measles, and other kindred diseases, upon a specific poison of animal origin, being a product of the diseased human organism itself. This assertion is proved by the characteristic feature which it presents

in common with all other specific diseases, consisting in the *immunity* from a second attack, imparted to the individual once affected; and this immunity from a second attack, which most strikingly distinguishes contagious diseases from all others, must be constantly kept in view in the study of the nature of the peculiar poison producing yellow fever.

The precise nature of the particular pathological changes which impart to the organism this immunity from the effects of the poison afterward, we are at present unable to determine. Perhaps the most popular view is that the poison alters the constitution of the blood in such a manner, and so permanently, as to be unable to make any further impression upon it afterward. It is equally difficult to say whether the changes are wrought upon the colored blood corpuscles in particular, or upon the plasmatic portion, the liquor sanguinis furnishing the nutriment to the organs and tissues of the body. Judging from the unusually rapid disturbance in the nutrition of the organism, manifested by the phenomenon of fatty infiltration and degeneration in a number of organs, and by the nervous disorders, the liquid portion of the blood would appear to be first affected; while, on the other hand, the facility with which the blood corpuscles seem to part with their hæmoglobin, as my observations have shown, indicates that these bodies must be likewise affected. For a number of years I have, in relation to the difference existing in the impression of the poison upon the blood in miasmatic and yellow fever, been in the habit of illustrating it in a somewhat homely manner by assuming that in the former the impression of the poison is only functional in character, and in consequence evanescent, while in the latter it is organic in its nature, and therefore permanent, and insuring the immunity from a second attack. But, though it appears to me possible that the immunity may depend upon certain permanent changes wrought by the poison upon the blood—difficult as it may be to explain—I cannot forbear to presume that similar changes may at the same time take place in the nervous tissues, rendering them in the same manner unimpressible for another attack.

Besides the immunity from a second attack, characterizing yellow fever as a disease produced by a specific poison, the clin-

ical symptoms, and, moreover, the anatomical changes observed after death, also speak for its peculiar and specific character. But as a specific disease cannot be produced in the human body by a general cause, it is obvious that the specific poison producing it must be derived from another human body affected by the same poison, and that this poison must, necessarily be a product of the organism of the affected individual. We may, therefore, presume that a *general* cause cannot produce a specific disease.

Yellow fever, when it first appears, never attacks a number of persons isolated from each other at one and the same time, but always, like every other contagious disease, starts from single centers, represented by the infected individuals; and the only manner in which it spreads is either directly from individual to neighboring individual, or indirectly, when the poison, emanating from the affected individual and adhering to clothes and other objects, is carried to a distant place and inhaled by another person. A number of centers may thus arise in different localities of a city, which, increasing in dimensions, may at last meet. In this manner, alone, the infectious poison is able to extend over a large city like New Orleans; and the surest proof of this assertion is offered by the slow march in which the disease walked over our city during the last epidemic. A purely infectious poison, on the contrary, being contained and distributed throughout the air of a city or locality, whether in the form of living organisms, or in the form of vegetable or animal effluvia arising from decomposing matter, will necessarily affect every person susceptible to infection nearly at the same time, and, in consequence, rapidly extend over a city or district. For this reason, the theory of a contagium vivum, or of the miasmatic or effluvial origin of yellow fever, is entirely inconsistent with the contagious character of the disease. But notwithstanding these existing circumstances, there are still a number of physicians who, while they recognize the contagiousness of yellow fever, entertain at the same time the incongruous idea that the cause of the disease is represented by minute living organisms, and for the sole reason, as they say, that this is the only theory upon which they can understand the increase of the poison, as nothing could spontaneously increase unless it were endowed with life.

I shall try to explain this subject by a suitable comparison. In every organized body, whether plant or animal, we meet with an albuminous substance which is regarded as the basis of life, and is generally known by the name of "protoplasm." This protoplasm, as we all know, forms the basis of all tissues performing a vital function, whether consisting of cells or fibers; and though it is not an organized body, it nevertheless represents living matter and is endowed with the power of appropriating other matter unlike itself—such as the nutritive matters derived from the food and contained in the blood,—and of imparting to it its own properties; or, in other words, of converting this matter into protoplasm. Now, it is upon this principle that all tissues and organs of the body not only rejuvenate themselves, but also increase in size. Thus, the protoplasm of a muscular fiber appropriates new matter and converts it into muscular fiber; the protoplasm of a ganglion-cell imparts its properties to the same new matter furnished by the plasma of the blood, and converts it into nervous tissue, etc. The new matter is in all cases the same, but it is the protoplasm of each particular tissue which represents the converting agency.

The specific poison, having entered the circulation, and being diffused throughout the blood, imparts, like the protoplasm, its own properties to this fluid, giving rise to all the pathological phenomena characterizing the disease. Still, it must be remembered, that the degree of intensity of the disease depends entirely upon the quantity of the poison taken up by the blood. If the quantity is sufficiently large to affect all the blood circulating through the body, the disturbance of nutrition would be so great as to interfere at once with all the vital functions, and the case would prove fatal very rapidly. We may, therefore, presume that, with some rare exceptions, in all cases of yellow fever the poison only affects a smaller or large portion of the blood, leaving to this fluid sufficient integrity to nourish the tissues and organs at least for some days, thus affording an opportunity to the organism to rid itself of the poison. This, however, can be accomplished in no other way than by means of the action of secreting, or, as we may say in this case, excreting cells, the same organs through which the organism rids itself of all other

foreign or wasted matter. Thus, the poison, already increased in quantity by that portion of the blood to which it imparted its own properties, is absorbed from this fluid by the cells of different glandular organs during its passage through the capillaries surrounding the individual minute glands. And, in passing through these cells, it again increases in quantity *by imparting its noxious properties to their natural secretion*, together with which it is finally eliminated from the organism. In yellow fever therefore, as in all other contagious diseases, it is in the secretions, especially those of the glandular cells of the skin and of the respiratory passages, that the noxious poison is contained, and with which it is removed from the system.

In some contagious diseases, the poison, before leaving the body, gives rise to certain pathological processes in the epidermic cells of the skin, manifested by divers exanthematous eruptions, which, in some instances, as in small-pox or the cow-pox, proceed to the formation of vesicles and pustules containing the poison in liquid form. Nevertheless, even in these affections, a portion of the poison leaves the body in a gaseous or vapor form, possessing the same virulent properties as the liquid contained in the vesicles or pustules. In yellow fever the poison emanates from the affected individual only in a vapor form, in which it may be communicated to another individual, or may adhere to surrounding objects, as clothes, bedding, furniture, etc. In the latter case, however, it may be supposed that while adhering to these objects it undergoes a certain condensation by drying, and in which state it is transported to distant places. But, as soon as acted upon by a certain degree of moisture and heat, it again assumes the vapor form in which it may be communicated to other persons.

In holding the view of the contagium of yellow fever or other contagious diseases being transmitted from one person to another through the vapor arising from the secretions of the skin and lungs, we need not presume that the whole vapor represented the noxious poison; on the contrary, we may concur with the opinion of Hiller, already cited, namely, that the ferments of putrefaction are represented by most minute proteïn-particles, which, being themselves in a state of putrefaction, are carried through

the air to the substratum to involve it in a similar decomposition. In the case of yellow fever, therefore, these proteïn-particles may be derived from the protoplasm of disintegrated epithelial cells of the glands of the skin to which the properties of the specific poison had already been imparted, and which, on their part, if entering the blood of another individual, are also capable of imparting these properties to the constituents of this fluid. This explanation corresponds with the theory that only solid bodies are capable of acting as ferments. Accordingly, the spread of the disease would be affected by the expansion of the vapor of the cutaneous secretion containing these particles into the air surrounding the patient, and inhaled by other individuals—and, furthermore, by the secretion adhering to the clothes and bedding of the patient, or to other neighboring objects; if the perspiration be profuse, as is generally the case, it will also be absorbed by the clothes and bedding.

In yellow fever as in small-pox the secretions of the skin are generally associated, as already remarked, with a peculiar odor. Whether this odor really represents the noxious principle, or whether, being somewhat ammoniacal in character, it is due to the decomposition of urea contained in the secretion, I will not pretend to determine. But, judging from its strength as it emanates from a single patient in a badly ventilated sick-room, we may form an idea of the facility with which the vapor, pregnant with the poisonous principle, may adhere to the clothes of visitors to be carried to distant places, or how easily it may communicate itself through the open windows to neighboring houses and people. But, in order to decide upon the probability of this mode of propagation of the noxious poison of contagious diseases, we should compare it with the manner in which odorous bodies impart their odor to the surrounding air, or to neighboring objects. Although nothing definite is known of the manner in which odors are diffused, or of the nature of the impression which they make upon the ultimate terminations of the olfactory nerves, it is nevertheless supposed (Cloquet),* "that many bodies possess the property of setting free from their substance exceedingly fine

* *v. Vintschgau*,—"Physiologie des Geruchssinnes"—Handbuch der Phisiologie, herausgegeben von Dr. L. Hermann, Vol. III, part 2, p. 161, 1880. (Still under publication.)

particles, forming a kind of atmosphere around the odoriferous body and becoming diffused into the surrounding air; their density diminishes in proportion to their distance from the odoriferous body. Accordingly, the air represents the medium by which odorous substances are spread."

"*Cloquet*, *Bidder*, *Valentin*, *Longet*, and *Poinsot*† have mentioned a number of conditions under which the odor is developed. There are, namely, odoriferous bodies the substance of which is incessantly, either partially or entirely, volatilized, while others only became odoriferous under certain circumstances. There are, for instance, plants which exhale only during daytime, or only during the night, or, even, only in the morning. Some plants emit odor when they are dried; a generally known example of this is found in freshly cut hay. Aromatic herbs possess in the dried condition only a feeble odor, but smell tolerably strong when moistened. Bituminous substances have no odor in the dry condition, but a distinct one when damp. The dampness appears, therefore, to favor the emanation of the odor, and still more, if the evaporation is supported by a moderate temperature; heat destroys the odor, and a low temperature arrests its emanation; these lower limits differ with different substances."

"*Liégeois* arrived at the conclusion that minute particles are constantly diffused from the odoriferous bodies, especially when they are in contact with water, in the atmosphere, and arriving at the Scheiderian membrane with the olfactory organ."

From these citations, then, we find that odors are produced by minute particles emanating from the odoriferous body to become diffused throughout the atmosphere, and their emanation is favored by dampness, especially at a moderate temperature. But there are also bodies of vegetable and animal origin which emit no odor, and yet manifest the same phenomena indicating the emanation of fine particles as from the odoriferous bodies. It is, therefore, not essential that a substance, in order to emit or set free minute particles, should be odoriferous. The tenacity with which odoriferous particles adhere to other bodies, especially such of a loose or spongy texture, is very great, so that in some

† l. c.—p. 262.

instances it is almost impossible to destroy the odor without the aid of water or chemical re-agents. The numerous examples of this kind, such as musk, castor, the secretion of the odoriferous glands of the pole-cat, assafœtida, etc., are sufficiently known as to require no further comment. It is only to the odor of the exhalations from the human skin, and to the tenacity with which the minute particles—emanating from the respective secretions from which the odor proceeds—must adhere to neighboring objects, that I wish to direct the attention. The most striking example of this kind is afforded by the dog tracing the trail of his master, or that of another animal in hunting. Many of the most remarkable instances are known and authenticated, in which dogs have traced the trail of their masters to great distances, even into neighboring countries, and which evidently show that a portion of the odorous particles contained in the exhalations of the master, must remain suspended in the air near the ground over which the trail passes, or adhere to the ground itself, notwithstanding the currents of air almost constantly present in the open streets or country. Nothing interrupts the trail but running water, which washes the particles away. In this instance, of course, the olfactory sense of the dog, perceiving these particles, is much more acute than that of man; this, however, does not invalidate the argument, as the particles must be there in order to leave an impression upon the dog's olfactory organ. Still more remarkable is the olfactory sense of insects, by whatever organ it may be represented, which attracts them to matters, which in many instances, even, possess no apparent odor, whilst they certainly must emit particles diffusing throughout the atmosphere to considerable distances to come into contact with the organs of special sense of these animals. It is a known fact, and one to which I can testify from experience, that in hot weather the odoriferous trail of a full-blooded negro may be perceived by the human olfactory sense for a considerable distance.

How strong and dense the exhalations of healthy persons even are, may easily be judged from the offensive odor of the air of small and closed sleeping rooms, if entered in the morning before they have been ventilated; and the tenacity with which these exhalations will adhere to linen clothes or other garments, may

be judged from the smell of those articles after having hung for some time in rooms overcrowded with people, as met with among the indigent classes of society.

Now, if we consider the amount of exhalation emanating from the skin and lungs of a yellow fever patient during the course of the disease, and moreover characterized by the peculiar odor already referred to, it must be obvious that the atmosphere of the room, even with proper ventilation, must become charged with the poisonous particles contained in the exhalation; and if there are several cases in the same house, the air within must eventually be contaminated with the poison; and attenuated as this poison may be, the particles representing it will surely adhere to surrounding objects, or escape through the open windows to be carried to neighboring houses. And, if we furthermore consider the complex nature of the intercourse kept up by the inhabitants of a large commercial city, like New Orleans, the facility with which a considerable portion of the poison, adhering to the garments of the nearer friends and visitors of a number of patients, will be carried to different places and houses must be easily understood by the philosophically thinking mind. For it cannot be supposed that every man, woman, or child, whose friends are lying sick in their homes with yellow fever, will break off their social and commercial intercourse with the rest of the people. In fact, this intercourse is so complex, and its threads so intricately interwoven, as to render the tracings utterly impossible, even to the sharpest detective, and at the very beginning of an epidemic. The many failures that have occurred in the attempt at tracing the first cases of yellow fever of an epidemic to their true origin are due to this complexity of social and commercial intercourse. But more especially to the fact, that it is almost always the *fatal* cases which first attract the attention of the authorities, while the *milder* ones, not seen by the physician, and perhaps not even recognized or suspected by the patient or his friends, themselves, and constituting the larger number, will get well without any medical aid. But, while the latter may not be noticed, they nevertheless reproduce and spread the noxious poison, as well as those terminating fatally.

In my paper on "The Nature of the Poison of Yellow Fever,"

published in the *New York Medical Journal*, May, 1879, I expressed the idea that the poison of yellow fever, similar to the virulent poison of putrid substances, increased in intensity with each individual through whom it passes. By a subsequent and more mature reflection, however, I have come to the conclusion that experience does not prove this supposition, as, otherwise, the intensity of the poison would soon reach a degree at which it would affect every person without regard to their degree of susceptibility—though it is not impossible that the virulence of the poison emanating from some individuals may be higher in degree than that emitted by others. But, even without this supposition, the quantity of poison emanating from any yellow fever patient, and possessing no greater virulence than that which originally entered his system, is sufficient to explain the spread of the disease.

As regards the transmission of the poison of yellow fever by ships from port to port which is, as mentioned before, a historical fact, it seems to me that even here the poison is generally carried by the garments and effects of the sailors or passengers, though I would not venture to deny the possibility of its being transmitted by the cargo, especially if the articles of which it consists had been exposed to the air of an infected locality. If cases of yellow fever have occurred during the journey, then there remains no doubt that the poison was carried by the clothing and baggage.

Regarding the decline and final disappearance of an epidemic of yellow fever, it is very generally believed that the noxious poison is destroyed by frost. The truth of this supposition, however, is only apparent, as it cannot be proved by any positive and reliable facts. On the contrary, it is a fact that, at New Orleans, the epidemics had almost always nearly disappeared when the first frost occurred, and that frequently new cases arise long after the appearance of this meteorological phenomenon. Thus, in the epidemic of 1867, I paid the first visit to my last case on the tenth day of December, a case which, though not proving fatal, was nevertheless a severe one, accompanied with jaundice and extensive hæmorrhages of the lungs, the patient being a foreigner who had arrived at New Orleans about two months before she was attacked by the disease. The same thing happened, as far as I

remember, at Memphis and other places situated more northerly than New Orleans, where the disease continued to prevail for some time after the appearance of the frost. Thus, the epidemic always reaches its maximum height a considerable time before frost appears, and there remains no doubt that its decline is chiefly owing to the decrease of the number of those persons susceptible to the action of the yellow fever poison, but also *to the lower temperature condensing the latter upon the objects to which it has adhered, and interfering with its evaporation, both from these objects and from the bodies of the affected individuals, and thus diminishing the chances of its being inhaled by other persons.* At the same time, the systems of those persons who thus far escaped the disease are reinvigorated with the approach of cooler weather in the month of October. These are, as I suppose, the chief causes to which the decline of the disease may be attributed, though the germ-theorists firmly believe that the frost kills the living germs, representing, as they suppose, the cause of yellow fever.

And to causes reverse to this may be attributed the re-appearance of the disease, especially in the year succeeding that of an epidemic, for it is very obvious *that during an extensive epidemic a considerable portion of the noxious poison must have remained adherent to clothing and to the walls of rooms, etc., as well as to mattresses and other bedding by which it had been absorbed with the perspiration of the patient*, some of which, after having existed during the winter and spring—perhaps unexposed to the atmosphere and light, and in a state of condensation—*may, with the returning heat of the summer, associated with moisture, be again evaporated and resume its activity.* There remains no doubt that in this manner the yellow fever epidemic, occurring in Memphis in 1879, arose from the clothing and effects of yellow fever patients from the epidemic of 1878, as soon as the shoemaker opened his box, in which they had been kept during the winter and spring. In the same manner arose those cases occurring in New Orleans in 1879, which, besides, might have increased in number to assume the form of an epidemic, if the board of health had not isolated them as far as circumstances would permit. All so-called sporadic cases arise in this or a

similar manner, and it is by no means improbable that the noxious poison of yellow fever, like that of small-pox, scarlet fever, measles, and other contagious diseases, may seldom become entirely extinct in places which it is accustomed to haunt, but constantly lurk about in dark corners deprived of fresh air, to resume its activity as soon as exposed to heat and moisture—and that, therefore, its fresh importation by ships is not always a strictly essential condition.

A further proof, however, that cold does not destroy the noxious poison of yellow fever, or of any other contagious disease, may be found in the known fact that low temperatures, on the contrary, preserve all organic substances. And to illustrate this fact we have only to point to the huge cadavers of the mammoth, buried for thousands of years in the ice-fields of Siberia, the flesh of which was so well preserved when discovered by the herdsmen of those icy regions, that they made use of it for feeding their dogs. But even if the cause of yellow fever were represented by living organisms, such as bacteria, it could neither be destroyed or killed by the cold of our winters, nor by a much lower temperature. This has been sufficiently proved by a number of experiments, but most forcibly by *Frisch*,* who exposed bacteria to a temperature as low as 87.5° C., below zero, a condition from which they revived with the thawing of the liquid.

Much has been said and written about certain meteorological conditions required for the activity and propagation of the yellow fever poison, such as a certain amount of moisture in the air together with a continued tropical heat, a deficiency of ozone, etc. Though nothing positive concerning these conditions has thus far been ascertained, I would not deny the probability of such particular conditions; for the fact that yellow fever has assumed the dimensions of extensive epidemics only at shorter or longer periods, while, on the other hand, it has frequently existed it the form of so-called minor epidemics, or been represented only by a few sporadic cases, shows that the same atmospheric conditions are not always present. Neither does the noxious poison,

* *Virchow u. Hirsch*, Jahresbericht fuer das Jahr, 1877, Vol. I., p. 285.

after its absorption into the blood, manifest its activity in the same degree in different persons, a phenomenon to be explained by the different degree of susceptibility which these persons possess to the effects of the poison, depending probably upon the particular state of their constitutions at the time when it is brought in contact with the latter. Thus we frequently meet with persons who have been exposed in many different ways to the action of the poison during several epidemics without ever being affected, but who, after the lapse of many years, are finally attacked by the disease. This was the case with myself; for while I escaped taking the disease in former epidemics, I became infected in 1878, while I was engaged in examining the blood of living patients at the Charity Hospital, and when, in order to obtain the specimen of blood, I was obliged to remain for some time in very close contact with the patient, thus inhaling his exhalations. In such cases the disease mostly appears in the milder form. Nothing is known of the nature of this singular immunity, whether it is owing to a peculiar constitution of the blood or of the nervous system at the time of exposure. Nevertheless, it has at times been taken hold of by the "non-contagionists," and forwarded as a proof of the non-contagiousness of yellow fever. Such an argument, however, must fall to the ground in considering that the same phenomenon is observed equally as well in scarlet fever, measles, and small-pox, etc., the contagiousness of which is universally acknowledged. Thus, a number of years ago, I attended a little girl about seven years old, affected with confluent small-pox; it was the most severe case of which I ever had charge, for the eruption extended from the face over the whole scalp, almost forming a mask, and was associated with considerable suppuration and a loss of the entire hair. In this case, remarkable as it may appear, the mother of the patient had during the whole course slept with it in the same bed, and the grandmother in the same room, without either of them contracting the disease, while the father by only once approaching the bed, at the time when the pustules were drying, rapidly caught the disease which fully developed upon him. As far as our knowledge extends at present, we are unable to offer any explanation of these contradictory facts, and it appears to me useless to indulge in empty speculations.

Although the customary assertion that native-born or long acclimated persons enjoy immunity from an attack of yellow fever has now been sufficiently proved as unfounded, it cannot be denied that, in most instances, acclimatization greatly ameliorates the severity of the attack. This is most strikingly observed in unacclimated persons, especially in those who have lived in northern latitudes, beyond the yellow fever zone, and who are generally the first victims to the disease. And it has become a general rule, corroborated by all writers, that the nearer a person has been born and lived to the North Pole, the more liable he is to be attacked by the disease, and the smaller his chances will be for recovery. The fallibility of this assertion, however, has also been shown by our experience during the epidemic of 1878, when the French, Italian, and Spanish population of New Orleans suffered equally as much, *if not more*, from the disease than that of Teutonic or Anglo-Saxon origin; even the Negroes did not remain exempt. Nevertheless, as regards non-acclimated or acclimated persons, it is not improbable that the air of the yellow fever zone may in some essential points differ from that of northern climes, enabling it to exert a certain unknown influence upon the constitution of the blood of those persons who breathe it, adapting it, so to speak, to the yellow fever poison. And to this circumstance it is mainly due that the rate of mortality during the epidemic of 1878 had been proportionately much larger in Memphis, Vicksburg, Holly Springs, and numerous other places, than in New Orleans, or even in other parts of Southern Louisiana. Besides this, the proportion of persons who enjoy an immunity of the disease, by having been previously affected by it, to the whole population, is greater at New Orleans than at the places above mentioned.

TREATMENT.

Although from the facts relating to the clinical phenomena, pathology, and probable nature of the specific poison of yellow fever, which I have presented to the reader in the preceding sections of this treatise, a rational course of treatment may be easily deducted, some additional remarks will be demanded to direct the attention of the practicing physician to certain apparently minor, but really important points.

The most simple method of arresting a disease, produced in a previously healthy organism by the noxious effects of a specific organic poison, would, of course, consist in the neutralization of the latter, inducing a cessation of the pathological processes—if not advanced too far—and a gradual return of the organism to its normal condition. Unfortunately, however, medical science has as yet not discovered the antidote of the yellow fever poison, and we must, therefore, look for another method of treatment, by which we may counteract the effects of the poison, and, moreover, assist in its elimination from the system with as little damage to the patient as possible—a treatment, which, if based upon rational principles, should correspond to the natural efforts which the organism is observed to make in the elimination of other infectious poisons. Numerous examples of this kind are constantly witnessed in the so-called critical sweats, diarrhœa and other discharges, or in the eruptions of some other contagious diseases; for, it would be difficult to prove that these processes, though pathological in character, do not represent the effort and the means by which the unaided organism chiefly rids itself of the foreign intruder, the morbid poison. In taking a simple case of intermittent fever as an example, we meet with many of the phenomena as are witnessed in the first stage of yellow fever, but which, in this instance, will disappear with the termination of the sweating stage. The most rational inference to be drawn from this fact, therefore, is that the poison, or, at least, *the product resulting from its combination with one or the other constituents of the organism*, is eliminated by a secretory, or rather excretory, process of the glands of the skin, or, perhaps, also by the biliary or urinary secretions. The same phenomenon may be observed in small-pox, when the fever abates with the formation of the vesicles, and ceases as soon as the whole eruption is out. From this assertion, however, it must not be inferred that an excessive secretion of the skin, or of the liver and kidneys, will at once eliminate the poison from the system, but it must be remembered, that every gland can, in its normal condition, only perform a certain fixed amount of labor, proportionate to the quantity of its parenchyma, and that any increase obtained by means of artificial or other abnormal stimulation must be followed by a

decrease of the normal amount. Besides, some other points and conditions associated with the febrile process, and incompatible with an excessive secretion, must also be taken into consideration. The secretory functions, therefore, should only be moderately promoted if possible, but never be stimulated to excess. The next object in view is to reduce the elevated temperature to near the normal standard for the special purpose of limiting the disintegration of the albuminous substances, which, as is believed by a number of pathologists, is promoted by an abnormally elevated temperature of the body. The third, and perhaps most important point to be observed, however, is to prevent or to allay every excitement of the nervous system.

In passing a review of the clinical phenomena and the pathological anatomy, we find that in all fatal cases the nervous system takes a most prominent part, and in discussing the general pathology of the disease I have endeavored to clearly demonstrate this fact. And it may safely be pronounced that if the integrity of the nervous organs were not disturbed, the organism, if left to itself, would easily get rid of the poison, and the fatality of the disease be removed. This assertion is fully proved by the thousands of cases which recover without any medical aid. The most dangerous of these nervous disturbances is the hyperæmia of the brain, which, as I have shown, is almost always observed in fatal cases after death. For this reason the physician should never neglect to study the condition of the brain of his patient, from the various nervous phenomena manifesting themselves during the course of the disease, particularly during the febrile stage, and by the proper therapeutic means and applications endeavor to keep the congestion of this organ confined within safe limits. The first condition, essential to the safety of the patient must, therefore, be *the perfect rest of the mind, as well as of the body.*

To secure the necessary rest of the mind for the patient is in many cases quite a difficult task for the attending physician on account of the pernicious custom, especially prevailing among the working classes, of the female friends and neighbors entering the sick-room, and going to and fro for the pretended purpose of inquiring after the condition of the patient, but in reality from idle

curiosity. I doubt not that many a case of yellow fever has terminated fatally for the want of the proper rest of the mind, and the proper nursing at the *proper* time. It usually happens that when there are too many friends the patient is neglected, and will have to pay the cost with his life. For this reason, the physician, after having taken the case in charge, should at once select one or two of the most intelligent and trusty friends, who voluntarily offer, to stay alternately with the patient, day and night, at least until the crisis is over. Much has been said about professional yellow fever nurses. I have never had a great deal of faith in these persons, as they are strangers to the patient, and frequently too meddlesome, imagining that they know better than the physician, and not carrying out his orders very strictly—not infrequently even supplanting them by their own notions. The most reliable nurses are always the nearer relations and friends of the patient, whose sentiments and interests are interwoven with his own. The impression made upon the patient, by seeing strange faces around him, can neither be favorable. In the epidemic of 1867, particularly, I observed the marked importance of conscientious and careful nursing in a number of severe cases, who, attentively nursed by their nearer friends and relations, recovered, while others, surrounded and nursed by strangers, succumbed.

As soon as the first symptoms of the disease appear, therefore, the patient should be confined to bed and made as comfortable as possible, both in body and mind. The physician should contribute to keep up his moral courage by representing to him his case as light as possible, and by explaining the importance of the rest of his mind. Intelligent patients I have usually advised to keep from using their mind at all, but merely vegetate, so to say, in trying to sleep, and to leave all cares to their friends. Regarding the sickroom, it is of the greatest importance that it should be well ventilated, as otherwise, not only the patient, but also those persons who nurse him, will be obliged to breathe his cutaneous exhalations containing the infectious poison. At the same time, however, every direct current of air upon the patient should be scrupulously avoided; he should be lightly covered, but his surface not wantonly exposed.

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In discussing the general pathology of the disease, I referred to the probable temporary increase of the biliary functions in the commencement of the disease, and, for this reason, the customary initiation of the treatment by the administration of a mild cathartic, for the purpose of unloading the portal circulation, must be recommended. There are a number of medicines which will answer this purpose. The most popular one in this city, both with physicians and the people, is castor-oil, and, if not rejected by the stomach, answers as well as any other on account of its quick action. A number of physicians administer a dose of calomel, combined with one or the other cathartic, such as jalap, etc. As the calomel produces to a certain extent a bilious stool, this remedy rather appears favorable, especially as we know from the autopsies that in almost all fatal cases the gall-bladder contained a dark brown, tar-like bile. The citrate of magnesia may also be used, though it is frequently rejected by the stomach. Subsequently, in the course of the disease, however, it is better to use an enema instead of the cathartic medicine.

The almost universal custom of giving a hot stimulating foot-bath for the purpose of inviting the blood from the brain to the feet, also, may be of service in the beginning of the disease, but with the necessary caution to be administered after the action of the cathartic, allowing the patient to remain undisturbed afterward. The foot-bath, however, will only prove of advantage in the beginning of the disease.

In order to promote the perspiration, it is customary—at New Orleans—to administer an infusion of orange leaves. These leaves, if possessing diaphoretic properties at all, possess them only in an inferior degree. They are simply used in Louisiana, because they are easily obtained. In the Northern States they would be replaced by "boneset," (Eupatorium perfoliatum), and in Germany by the flowers of "chamomile." And as such infusion, in order to produce a diaphoretic effect, should be administered while hot, but which, in most cases, is neglected, the benefit derived from the orange leaves is only imaginary; even if the cold infusion is taken with the view of allaying the irritation of the stomach. Besides, the administration of hot infusions at a

time when the patient suffers already from the excess of heat, both from the weather and the disease, is hardly practicable, as it really contributes nothing to the comfort of the patient. Perhaps one of the most efficient and harmless remedies is the "neutral mixture" with the addition of a little ($\frac{1}{16}$ part) sweet spirits of niter, given in such doses as to keep the skin only in an evenly moist condition, which, however, may be discontinued while the skin remains moist, but resumed as soon as the perspiration ceases, and the skin becomes again dry. This mixture is tolerably well borne by the stomach, whilst the spirit of niter, besides, may slightly stimulate the kidneys. But as with the rise of the fever the thirst of the patient rapidly increases, he finally also refuses this remedy, and demands ice-water, which, if given in proper doses, appears to suit the occasion as well as any article the apothecary could furnish. Unfortunately, however, the thirst of the patient is so great that he loses his self-control, and will fill his stomach with the cold liquid if not interfered with by his nurse; then, small fragments of ice suffered to slowly dissolve in the mouth may answer the purpose.

[TO BE CONTINUED.]

ARTICLE VII.

MASSAGE. By JAMES I. TUCKER, A.M., M.D. Chicago.

Inasmuch as massage is attracting considerable attention now-a-days, and is thought by many to be a notion of modern times, it occurred to me that the following medico-historical data * might be no less interesting to your other readers than they have been to me.

In India the massage method was in vogue at least two thousand years ago, for a Greek historian, who was in that country 300 years B. C., related that at that time there existed among the Brahmins an order of physicians whose treatment consisted in regulating diet, enjoining abstemiousness and outward manipulations. At the present time, in the valley of the Ganges, there is an order of Brahmins, whose method of treating disease is a hygi-

* See Gartenlaube Heft 14, 1881, p. 445.

enic rubbing and shampooing after the bath. The patient lies outstretched upon a lounge, and the Brahmin kneads his limbs as if they were dough. Then he strokes the body lightly with the palms of his hands, perfumes and anoints him, and concludes by bending and stretching the neck and joints of fingers and toes.

The same method is in vogue in the Orient, especially among the Turks. The beneficial effects of massage were likewise known to the ancient Greeks and Romans, for after their bath they had their slaves rub, knead and anoint them, and Hippocrates considered it important that the physician, among other things necessary to the mastery of his art, should understand massage. Asclepiades, who lived one hundred years before Christ, was likewise an advocate of this method of healing.

It was a Swede named Pehr Hewrik Ling, who, while founding a system of gymnastics upon purely anatomico-physiological principles, introduced passive motion. This occurred in the second decade of this century, and the students of Ling brought the massage method to great perfection. For a long time all kinds of passive motion, like stroking, patting, pounding, kneading, rubbing, pinching, stretching, etc., have been applied with great success.

Of late a Hollander named Metzger, who has achieved brilliant success by the help of massage, attracts to himself both the learned and the laity, some to place themselves under treatment, and some to learn his method.

His method consists principally of four kinds of manipulation, to wit:

1. *Effleuvage:* slow and gentle stroking with the palms of the hands.

2. *Massage à friction:* forcible stroking and rotary rubbing movement in alternation, either with one hand or with both hands.

3. *Pétrissage:* kneading.

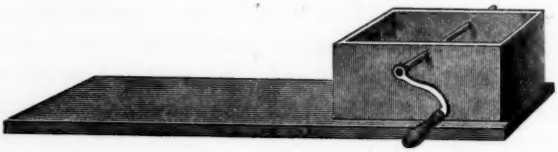
4. *Tapatement:* pounding, beating and slapping with the flat hand or its edge, and with the closed fist.

So important an adjunct to the medical art as massage, that rests upon a scientific basis, and endorsed by so many German, French, English and American physicians, including Esmarsh, Charcot and Mitchell, should not be overlooked by any of the members of our profession.

ARTICLE VIII.

A PRACTICAL BANDAGE ROLLER. By NELSON H. CHURCH, M.D., Chicago.

In the *Medical Record* of July 16 inst., appeared an item entitled "An Easy Way to Roll Bandages." I write to approve of the method, having used the same for ten years. Last winter I thought of improving upon the plan. I made an apparatus, which is not much of an improvement, though it is decidedly more convenient. It is described as follows, viz.: A wooden



box, measuring on the inside 2¼ x 3 inches, and two inches in depth (or deeper if desired). Two holes were made through the bottom of the box, one in the middle and one near the end. These holes were large enough to admit a wire, as large as a No. 5 catheter. Now take a piece of board one inch thick, four or six inches wide, twelve or eighteen inches long; make two holes in the end and corner of the board to correspond with the holes in the bottom of the box; through these holes put the wire which has previously been bent at right angles—one angle to be half an inch long, the other to be one and a half inch long and to have a thread cut on it (which can be done by a gunsmith); have the short angle go part way through the board, the other to go through and fasten with a screw nut, or, what would perhaps be less trouble, make a groove in the bottom of the board deep enough to accommodate the long end of the wire when bent to the proper angle. This manner of fastening is preferable to screws, because it will not work loose nor split the box. It is not necessary that the box should be fastened to a board; it is only suggested to save mutilating a desk or table. One can sit on this piece of board

while rolling the bandages. When one has a table that can be used for the purpose, holes can be bored in one end in the corner, the box fastened and left *in situ* for immediate use. I prefer to have the roller fastened to a table because it is firmer than the board, and one can handle the bandage more readily. The size of the box will admit of rolling bandages wide enough and long enough for any ordinary purposes. The crank is better made of wire the size of a No. 5 catheter and put through the middle of the box, in either diameter, about half an inch from the top; this will admit of rolling a long bandage without touching the bottom.

When rolling a bandage make pressure on the crank, keeping it well in place, and hold the bandage quite firmly over the edge of the box. The end of the bandage should be slightly wetted before commencing to roll, as that makes it adhere to the wire just enough to prevent slipping. After the bandage is wound, pull out the crank, or if the raveling should hold the bandage fast to the wire reverse the crank a few times and withdraw it.

With the smallest amount of practice one can roll a bandage very quickly, very evenly, and very firmly.

PRIZE ESSAY.—The Committee of Selection appointed by the chairman of the Section on Practical Medicine, Materia Medica and Physiology, at the recent meeting of the American Medical Association, have selected, and hereby announce, as the subject for the prize to be awarded in 1883, the following question:

What are the special modes of action, or therapeutic effects upon the human system, of water, quinia, and salicylic acid, when used as anti-pyretics in the treatment of disease? The essays must be founded on original experimental and clinical observations, and must be presented to the chairman of the committee of *award* on or before the first day of January, 1883.

N. S. DAVIS,
H. D. HOLTON,
W. B. ULRICH,
} Com. of Selection.

Clinical Reports.

ARTICLE IX.

SURGICAL CLINIC OF PROF. ANDREWS. AMPUTATION OF THE KNEE JOINT.

Amputation at the knee joint is an operation which has been unaccountably neglected in America and, in fact, the world over.

Dr. Brinton, in 1868, published (*Am. Jour. Med. Sciences*) statistics of only four hundred cases of this amputation as a result of inquiries into the operations of the whole world. Ten years later I reported the search among the published statistics of both continents showing five hundred and fifty published cases. Since that time, although the operation has made progress in public favor, it has remained somewhat rare.

The following cases recently occurred in Mercy Hospital:

11,134. Female, aged 48. Entered hospital with a severe form of tertiary syphilis. The right leg was a mass of ulcers. The left one was similarly but less severely affected, being spotted with suppurating tertiary sores. Similar ulcers existed on other parts of the body to a limited extent.

The treatment consisted of iodide of potassium and local antiseptic dressings for twenty days, when it was found impossible to save the limb and it was amputated through the knee joint, after the method of Dr. Stephen Smith.

The following cut shows the line of external incision in the bilateral flap amputation recommended by him in his *Operative Surgery*:

Beginning about one inch below the tubercle of the tibia and, cutting to the bone the knife is carried downward and somewhat

forward around the curve of the limb when it is turned somewhat backward and the incision terminates in an upward direction just beneath the line of articulation in the popliteal space. The flaps are similar except that the inner one is made somewhat more redundant to allow for the greater prominence of the inner condyle.

These flaps are made to include everything down to the bones, and when they have been carefully separated and raised, the ligaments of the joint are divided and the leg separated, only a scalpel being required to complete the cutting.

The older plan of removing the articular cartilages, or of sawing off the condyles is to be condemned in emphatic terms. In practice it is found that the much dreaded inflammations of the remaining joint surface is not met with if antiseptic treatment is effectively carried out. The patella is also left *in situ*, and never transposed to the fossa below as formerly recommended.

This patient left the hospital twenty-five days after the operation with the stump almost entirely healed by first intention.

Case 11,339. Male, aged 40. This man suffered from long standing necrosis in the upper part of the tibia of his right leg. In 1864, while a soldier, after great fatigue and exposure from sleeping upon the ground, he began to suffer from an inflamed knee and great lameness. This continued about one year, when an abscess formed upon the external aspect of the limb just below the joint, and there had been a running sore ever since. The head of the tibia was much enlarged and there was such extensive necrosis in its upper third that I agreed with the patient in preferring an amputation to an excision of the joint.

The amputation was performed as in the last case, leaving the patella which showed no implication in the disease, and the joint surfaces with all the cartilages of the femur. The external

condyle of the femur showed plainly by its inflamed condition, that there had been synovitis; nevertheless, no part of it was removed, and not much trouble arose from this fact in the healing. Much time was taken to ligate all the small arteries about the joint, because of the excessive anastomoses and vascularity of the parts from the previous inflammation.

The operation was performed under spray, and the wound treated with Listerian dressings carried high up the thigh.

A very speedy recovery was obtained under anodynes, iron and cinchonidia, with a temperature at maximum during recovery of 101½° Fahrenheit.

Five weeks after the operation a slight hæmorrhage occurred from the wound, and at the same period severe pains and swelling over the outer condyle; a small abscess had developed at this point and was opened down to the bone. No caries appeared to be present. This opening was kept widely open, and there was no recurrence of the pain. Two weeks and a half later, or seven and one-half from the time of operation, the patient left cured.

Case 11,256. Male, aged 30. Attempting to climb upon a moving freight train, this man fell under the wheels and had his left leg crushed off at the upper third.

An hour after the accident his pulse was 120. An amputation was decided upon at the knee joint, after some hesitation in regard to the vitality of the tissues destined to compose the inner and external flaps. The skin and superficial fascia were found loosened at one part as high as the lower third of the thigh from their attachment to the parts within. Ultimately abscesses formed as high as the upper third, which caused some trouble but no serious delay in the healing.

Three hours after the accident the knee-joint amputation, as above described, was performed by Dr. E. Wyllys Andrews. The flaps were scanty and the external one bruised and torn.

Forty-eight hours later a small triangular segment of this flap was found to have mortified, and the wound gaped open at this point leaving the condyles of the femur exposed.

The temperature was for several days 103° at evening, and pulse 130.

The abscesses in the parts above were thoroughly carbolized

by means of injections through a soft rubber catheter beneath the flaps and through the counter openings.

At the present time—seven weeks after the operation—the exposed parts are covered with rapidly-growing granulations, and the contraction of the cicatrix bids fair to draw in the surrounding parts so as to furnish a more or less complete tegumentary covering to the stump.

The advantages of this operation are such as to commend it most strongly where other indications are suitable. First of all, it is far less dangerous than amputation in the lower third of the thigh. A greater length of limb is at the same time secured and one which gives the best support to an artificial limb. In this respect it is superior even to the stump after a leg amputation.

It is the only stump upon which the entire weight of the body can be carried without pain or ulceration. The scar left is surprisingly small, and is in the posterior aspect where it does not interfere with the use of any artificial appliance. There is no cicatrix upon the end of the stump at all. No bone being divided in the operation, which requires for its execution only a strong scalpel, the danger of osteo-myelitis is not to be counted among the risks attending it. The supposed dangers from leaving in the wound a large synovial surface are from *a priori* conclusions and are wholly mythical.

NOCTURNAL INCONTINENCE OF CHILDREN.—Prof. S. D. Gross, of Philadelphia, advises the following formula:

Strychniæ	gr. j.
Pulv. cantharides	grs. ij.
Morph. sulph.	grs. iss.
Ferri. pulv	scr. j.

MIX. Make 40 or 50 pills or powders, *pro re nata*. Sig.—One three times a day to a child ten years old.

This prescription will speedily relieve the irritability of the bladder, especially if conjoined with such means as a cold shower bath daily, the avoidance of irritant food and late suppers, the patient lying on the side or belly, and taking care to drink nothing for the few hours preceding sleep, and to empty the bladder on going to bed.—*Mich. Med. News.*

Hospital Reports.

ARTICLE X.

COOK COUNTY HOSPITAL.

CASE OF HYDROPHOBIA. Service of DR. NORMAN BRIDGE. Reported for THE CHICAGO MEDICAL JOURNAL AND EXAMINER, by H. D. VALIN, M.D.

Peter D., a Norwegian laborer, on admission to the hospital Aug. 10, gave the following history: Father died after a surgical operation for necrosis of the tibia; mother and two sisters are living and healthy. Three children died during infancy. Patient was sick when three years old, but does not know the nature of his sickness. Had jaundice seven years ago. He has been stabbed about the face, head, back and extremities in various brawls. Has been a hard drinker, using beer to excess, and occasionally drinking whisky. Last March he was bitten on the left hand by a small dog, which was killed immediately, as a matter of course. The wound healed within three days, without a physician's care.

Monday, Aug. 8, patient worked as usual. During the afternoon he noticed that his head felt heavy, and soon began to ache, in the posterior part especially. He ate his supper, and rested well that night.

The 9th, in the morning, he ate some breakfast and drank one cup of coffee. He tried to drink another, but could not, and felt sick, with a severe headache. He lay down, with the intention to go to work at noon. At 9 A. M. a friend brought him some beer, which he attempted to drink, but could not. He then tried water, which at once made his throat feel as though he were chok-

ing. After that he could not look at water without the same feeling. Aug. 10, at night, he could not sleep well, and the 11th, in the morning, he felt worse. He said that he was smothering, and his head ached. Went out to see Dr. Fenger, and the wind made him almost wild; he had to stop often to get breath. He could not drink water or look at it without having spasms of the muscles of the throat come on. States that the place where he was bitten has caused him no pain at any time.

On admission, the 11th, he complained of pain in the head, at the base of the posterior part. A slight draught causes tremor of his whole body and a smothering feeling. Upon attempting to drink water he makes convulsive movements, and states that he feels as though he was smothering.

He had altogether, in the afternoon of the 11th, 80 grs. of chloral, in 12 and 15 grs. doses. At 7 P. M. he took 9 grs. of bisulphate of quinine. The retinæ were normal, as per ophthalmoscopic examination of Dr. Jacobson. At 10:30 P. M. he took fifteen grs. of chloral, and soon after passed his water.

At 10:45, took a drink of water by himself, a marked improvement, seemingly due to chloral. 11:30 chloral gr. xv.

Aug 11. 12:45 A. M., pulse 118. Patient cannot sleep, and takes his medicine with difficulty. All his symptoms were more marked.

2 A.M. Pulse 120.

3 A.M. A dose of chloral.

3:30. Patient was raving. Took another dose of chloral. No sleep during that night.

9:30 A.M. Chloral, gr. xv hourly. Bowels moved.

10. Pulse 128, temperature 101.8.

12 M. Half a grain of morphine hypodermically.

3:40 P.M.

℞. Potassii bromid.....3 j
Chloral hydrat.....gr. xv M.

one dose.

4:20. Attendant had to put on the straight jacket to keep the patient quiet.

4:25. Chloral, gr. x hypodermically.

4:53. $\frac{1}{1}$ grs. of curare was given hypodermically. The

spasms came on and passed away every few minutes. Patient tried to drink water through a rubber tube four feet long, and required the goblet to be held out of sight. He did not succeed in drinking. He assumed a quarrelsome countenance, and said that he would not drink, because that would kill him, he thought. He made great efforts at expectorating, and his cough had that peculiar roughness which is also noticed in delirium tremens, and not quite unlike that of croup, though it did not resemble much the barking of a dog. Although the patient's hands and feet were tied, and he spat on his guardians, he made no effort at biting, which would naturally suggest itself in such circumstances. Although raving a part of the time, he would also occasionally speak rationally.

5:45 P.M. $\frac{1}{6}$ gr. curare hypodermically.

5:50. Chloral hydrate, gr. v., in the same manner. The patient seemed to quiet for a little while under the influence of curare, and he looked as if the disease had as yet but slightly changed his features.

6:25. Curare, gr. $\frac{1}{6}$ hypodermically. Patient soon quieted down, but still muttered.

6:35 P.M. Pulse 180. Temperature 109¼ in the rectum.

7 P.M. He breaks off the straight jacket. Pulseless at the wrist.

7:05. Aqua ammonia, ℥ss hypodermically, and also by inhalation.

7:08, temperature 109.5, pulseless.

7:10. Dead.

This case has this remarkable character, that the incubation of the disease lasted over four months, a time at which some authors believed that all dangers are passed. The duration of the disease was the average. The treatment with chloral and curare seemed beneficial.

It has been alleged that cases of death by curare have taken place, but in this we see that ten minutes before death the patient had strength enough to break off the jacket, and did not die paralyzed.

Foreign Correspondence.

Article XI.—The London Medical Man.

Messrs. Editors :—In these pages I will endeavor to describe to you, so far as lieth within me, the London Medical Man in all his stages, from all his aspects. I will touch especially on his education—preliminary and medical—his earlier years of practice, the divisions of practice, his finances, his way of doing business, his manner of living, discouragements, enjoyments and social position. I speak from five months residence in London, in daily contact with the London medical man and his work in the various hospitals, schools of medicine, and private practice, and his social world. Also, from having interviewed men in the various divisions of the practice, and the various stages of life; also those who look on and see from an impartial and unbiassed standpoint.

To have a good basis, we will allow that he is born at full term, the legal child of virtuous parents, a vertex of the 1st; that he cries lustily; that the cord is tied and cut *secundum artem;* that he is carefully washed, and, as soon as the doctor leaves, given butter and sugar, and, possibly, a "dhrop of spirits." All this he survives, as does he also, cake, buns, beer, pickles, and "just what we ourselves have;" conquering, also, the dessicating cord, the thrush, the ruthless vaccination, the perils of the first summer, teething, measles, whooping cough, scarlet fever, chicken-pox, mumps, knives and breeches. He lives on despite the school-board, the school-marm or master, the vitiated atmosphere of the school room. Rolls, rules, after hours and lessons affect him not. He is born to be a doctor, and not to die in infancy or childhood as do thirty per cent. of his fellows. He

follows this schooling till the age of sixteen years. He becomes eligible to register and make his matriculation examination and enters the study of medicine. He either does this, or, as a minority do, goes on taking the degree of B.A., or B.Sc., at Oxford, Cambridge, London, or some other university.

Just here may be given the two great divisions and the four subdivisions of those who in the United States would come under the title of doctor. First are the physician and surgeon. Then the general practitioner of medicine, the consulting physician, the general practitioner of surgery, and the consulting surgeon. These four divisions are quite definite.

The general practitioner of medicine generally does not have a literary degree, but passes his matriculation examination at the Royal College of Surgeons, Royal College of Physicians, Apothecaries Hall, one of the Universities, or at the Hospital Medical School at which he enters, or one of the numerous other places. As an example of their nature I will quote the requirements of the matriculation of the Royal College of Surgeons: a certificate of having been registered by the General Medical Council; writing from dictation; English grammar; writing a short English composition; arithmetic, geography, English history, mathematics; algebra, through simple equations; Euclid, books I and II; translation from Cæsar de Bello Gallico. These are compulsory, besides which are a number of optional subjects. His studies are only recognized from the time of his registration. He may have studied any length of time before his name is placed on that public register but it is no good as to time. He then either goes to some recognized provincial hospital for eighteen months, or goes as the pupil of some legally qualified and registered practitioner for the same length of time. Said practitioner must be connected with a hospital, dispensary, or hold a poor-law appointment, so as to be enabled to give his pupil instruction. Not more than eighteen months of such study as the above is recognized by any of the licensing bodies. Such study is becoming more infrequent and unpopular, the student generally going immediately into his hospital medical school. It is strongly recommended that the pupil pass his preliminary examination before endeavoring to do anything at medi-

cine, so as not to have too many irons in the fire. At least two and one half years of the four years required must be spent at a hospital medical school, including three winter and two summer sessions, the former six months the latter three months each. Thus the English *annus medicus* equals nine months. A hospital, to be recognized by the licensing bodies, must have 150 beds. All of the first and a greater part of the second year he spends with the bones, the anatomy, in the physiological laboratory and dissecting room, practical pharmacy, lectures on materia medica and botany, peeping occasionally into the wards and out-patients' rooms, and of course being a regular attendant on the "capital operations," and prepares for "the first college exam." This passed he enters regularly into the study of medicine. He clerks, or, as the English say "clarks," with the *a* as broad as you can make it. This he may do in either the wards, out-patients, or special departments. It consists of examining the patients, taking notes or histories, and generally assisting the physician in charge and endeavoring to answer all the questions put to him by said physician. This he must do for six months. He hears lectures higher in the scale, including medicine, surgery, midwifery and gynæcology, instruction and proficiency in vaccination, pathological anatomy, forensic medicine, and does post-mortem work. Having completed four years study under the above regulations, he is permitted to come up for his pass examination. A large per cent. fail the first time and are remanded for three or six months. There are nineteen licensing bodies in Great Britain, and he has his choice. None of the applicants for license are allowed to be examined by their own teachers. A majority do not get into practice for five years. The general practitioner after receiving his license generally settles down to business in some modest house in a modest street, and takes unto himself, probably a modest wife. He usually takes the license of the Royal College of Physicians, or the license of the Society of Apothecaries, with probably a surgical degree. During the first two or three years after this he takes the M.D. degree, from Oxford, Cambridge, London, Durham or some other of the universities.

As to the consulting physician the differential diagnosis is this:

He is a little more ambitious in the start and his ambition and patience grow and increase. He also usually commences with more money—the average student probably commencing with from $2,000 to $3,000. He takes the B.Sc. from the London University, or is a graduate in arts from some of the universities in the majority of cases. Usually spends five years before he takes his M.B., or M.R.C.P. Practice is a thing he does not bother about. He hangs around his hospital or other hospitals. If sufficiently influential, or expert enough in wire pulling, he gets a house physiciancy, may be a house surgeoncy, or he may go through both these positions, also that of midwifery, assistant or surgical registrar, or chloroformist. He looks out for assistancies in the special hospitals of various kinds. He endeavors to put in about three years in work of this kind, making in all probably eight years before his brass plate goes on his door. During these years his love for the study which first engaged his studious attention is paramount, and he takes on letters whenever possible. For instance, a young man who recently, in London, attained to a position of honor, writes after his name, A.M. M.B. (Cantab.), M.R.C.S., Eng., and lives. The love of the London medical man for the alphabet is wonderful.

The general practitioner of surgery and the consultant in surgery, differ from their brothers, the physicians, in their education only, as you can easily imagine. They pay more attention to surgery than to medicine, though they generally take a medical degree. The consultant, especially, is very jealous of his title of "Mister." If you wish to see his indignation rise just call him "Doctor." Consulting surgeons are Fellows of the Royal College of Surgeons. Throughout the lectures each man's presence or absence at each lecture is registered, and if he does not attend a good portion his papers are not signed, unless the student can, by a few judiciously placed pennies, bribe the man who keeps this register. Class "Exams" and "Grinds," are held with regularity. As to fees, the perpetual ticket entitling the holder to all the hospital practice and medical school work throughout, in one of the best hospitals in London, is 132 guineas ($670.56). This even does not entitle him to his degree, but he must pay, for the license of the Society of Apothecaries,

$30; for the license of the Royal College of Physicians, $76; for the membership of the Royal College of Surgeons, $107; and on failing to pass, a portion only of the fees are returned. The high fees are somewhat offset by large scholarships. These, varying in value from $250 to $700, are won annually by competitive examinations, the poor student's only chance. I might add, what does not appear in the prospectuses of the London hospital medical schools, but which forms, to the student at least, an important part of the curriculum, there are boat-racing, foot-ball, cricket, and lawn tennis. It might be well to add to the curriculu of the medical schools of the United States from the London schools, botany, forensic medicine, comparative anatomy, and mental diseases. It is not necessary to add boat-racing, cricket, etc. It would be well were the required time of medical study in the United States, instead of three years, four years as in London. It would be better were it also made a requirement that the students study with as much diligence throughout the four years as they do throughout the three. Occasionally, an Englishman goes abroad to study medicine, but not often, for there is a prevailing opinion here that what is not known in London is not worth traveling expenses. As an example of this opinion, a medical man of Edinburgh inaugurates a new system and secures its introduction and practice in Germany, France, America and elsewhere, yet is obliged to pack up his household plunder and come down to London, and become of London, before he can introduce his system in London. There is, however, at Oxford a scholarship, the taker of which must have spent eighteen months abroad.

Having the men now educated, we will start them into practice. The general practitioner of either medicine or surgery generally starts in pretty readily. He rents a house at $250, $1,200 or $2,000 a year, and commences. Rather oftener than otherwise, he is in partnership with one or two others, and the firm, if a good one, will make $10,000 to $30,000, less costs of medicine; one man alone, making the half or the third of this amount. His hours are rather irregular, being liable to be called at any time, bnt if alone he tries to keep some hours. If in a firm some one is generally at home. He does, as a rule, his own

bookkeeping, and sends his bills out by mail quarterly or semi-annually. His fees range from 62½ cents to $2.50, with a preponderance towards the former.

The consultant's life is quite different. He thinks not of practice, but still hangs about his hospital medical school and special hospitals. He takes such positions as casualty physician, demonstrator of anatomy, practical surgery, tutors students, assists others, takes the places of members of the staff when absent, maybe gets a lectureship in the hospital medical school, next a situation as assistant physician or surgeon—his work in the out-patients' departments of the hospital. All this time his profession has brought him but the smallest stipend. After having been assistant physician to the hospital for probably fifteen years, some one above him dies, gets old, or angry, and he steps into his place. A consultant in London must hold a hospital position or his profession will amount to *nil*. During those years of trial—the first fifteen—he must keep up appearances, and seem, at least, to be making money. Many make something at other employments; for instance, writing articles for journals. Many have a private fortune to begin with, and their income keeps them. These terrible fifteen years he waits in his house, the best he can afford, and generally situated in the West End, and does not make $500 per annum. The surgeon generally works in sooner than the physician, for people would as willingly, perhaps more so, see a young man with good nerves coming at them, knife in hand, as an old, gray-headed man with glasses, and hands like an aspen leaf. The old surgeon often throws away the knife, and only gives opinions. A consultant always charges a guinea fee,—"a guinea or nothing." A few of the leaders, in order to "weed" a large practice, charge two guineas, and it has the desired effect. The consultant's fee is cash. He is called in, his opinion given, and the guinea taken. Such is the custom, and it is rarely broken. He keeps no books, collects no fees, writes no duns. The consulting physician is bound by his diploma not to go into partnership, nor into court for fees. Upon doing so he relinquishes his diploma, also his standing; hence, he never does it. Of this the laity, of course, are not generally aware. The consulting surgeon is really under no such restrictions; yet practi-

cally, from the general opinion of his fellows, he is as closely bound to the same rule as his brother the physican. A general practitioner of either medicine or surgery rarely grows into a consultant. In the occasions in which he does, he is expected to, as it were, wean himself from general practice by a season of total abstinence, as to practice, for two or three years. His office hours are, in the morning, until one P.M.; in the afternoon he goes out to see patients to whom he is called, usually going in a close carriage, with one horse and a coachman. The evening he spends in his study, or as seemeth to him best. In his money-making days, the consultant makes an average annual income of probably $6,000 to $15,000, with the leaders, who can be enumerated digitally, making $25,000 to $60,000 per annum. If called out of town he charges at the rate of $3.39 per mile—no more, no less. A few of the operating fees are these: calotomy, amputation hand or arm, $125 each; amputation leg, $250; lithotomy, $250. The universal shingle is a brass door-plate, which is large proportionately to the want of knowledge on the part of the practitioner. Some men embark in dispensaries. Charitable dispensaries are generally considered honorable. Paying dispensaries are frequently a little crooked. Fees are low. For instance, the window will contain the following list of prices:

Advice with medicine	$ .25
Visits	.25
Visits for one week daily	1.00
Midwifery, with attendance, one week	1.25

Homœopaths thrive. There are not many, but what there are are well kept by the pampered class, the nobility, who can't bear to take nasty things. A homœopath must get regularly qualified through some of the qualifying institutions. One may make loud assertions that he intends to practice homœopathically, and present himself and get his qualification from a regular licensing body. There is a homœopathic school in London, but it cannot qualify.

With reference to the social standing of the medical men of London, I wish to express myself with great caution. Few have received from them more kindnesses, more courtesies and more favors than I, an utter stranger in a strange land. Few have for

them a higher regard. Such being the fact, I hope and think it will keep me from doing them any injustice, or by mischievous words or hasty sentences conveying any impressions reflecting disparagingly against them.

In judging of their social status, one has carefully to bear in mind the peculiar forms of English society. Here men are classed after the most arbitrary fashion. One holds his social position not so much with reference to his merit and attainments as by certain accidents and adventitious relationships. Birth, kinship, wealth, title, station, even locality, chance surroundings do, as an American must think, decide most preposterously the social status of an individual. It is extremely difficult to fully understand this, unless one has resided in this country and closely studied its peculiar social conditions. The members of the medical profession form no exception to this idea—that all men are *not* born free and equal. A London medical friend recently stated to me that the physicians and surgeons in a certain portion of London, a large portion, are considered "pariahs, snobs, cads, kicked and cuffed by every one." A sad picture, drawn by no amateur hand in these few words. And yet there are men practicing in that area whose attainments, worth and refinement of character are such as to entitle them to the very highest social position in any country where society is not outlined by such whimsical distinctions as rule here.

This reminds me to call your attention to the fact that about two years ago a London correspondent for a Philadelphia medical journal got into rather hot water with the profession here by making a similar statement—indeed, saying in so many words that the medical man in London was a social pariah. This was at once hotly contested by some of the brethren here, but the writer maintained his ground vigorously, and I am inclined to think successfully. Undoubtedly the physician does not rank so high, socially, as he deserves, but this even requires a qualification. If the medical man be compared solely with those who hold their positions not merely by extreme wealth, titles or birth, but by education and merit, he would rank high. A few have been knighted, and are Sir Medicus, and their wife Lady Medicus, and walk out to dinner before their less honored fellows. In

many cases, however, this knighthood is given with restrictions; for instance, that it cannot descend but by direct male line from father to son; no atavism allowed. Sir Medicus of course goes among the swells, who honor his presence, if not very heartily, at least by sufferance. He gets this title usually from having attended some member of the Royal family of this or some other kingdom, empire or principality in severe illness. Of course merit is presumable on his being chosen to attend in said illness. One London surgeon received his title for passing a catheter on the King of the Belgians. There are now six Sir Medici in London. The first medical man so honored was Sir Hans Sloane, in 1716. They are distinguished from the plebeians by wearing a little red band on their coat sleeves, or when out riding on their carriages. This is the symbol of all knights created since the union of the kingdoms. A created knight, too, of course is not on the same equality as one whose line runs from him who supplied James I with forty men armed and equipped, or with £1,000 to subdue the Ulster revolt, and thus getting his knighthood; not even with the batch created by Charles I to settle and mobilize Nova Scotia, which they took care not to do. Some claim that the medical man here in his various divisions ranks socially in the same scale with the various divisions of the legal profession. Many others say not; but that the rank is: clergy, law, medicine.

I am sure that the London medical profession in America would occupy a first rank, socially compared, in the eyes of his fellow men. The American medical man is much ahead of his London brother. The same is the case in Dublin and Edinburgh. In these two cities the medical man is often a social light compared to the London man. This can be explained somewhat by the fact of London being the largest and greatest city in the world, and of course the greatest men of all classes, to an extent, come here—while in Dublin and Edinburgh the medical men have not such strong competitors usually as in London. But then it is also true that London has the greatest, at least many of the greatest, lights in the medical profession.

In illustration of the position London medical men hold among their fellows, one fact. During the recent long pending of a

bill for the abolition of vivisection, in Parliament, the entire medical profession of the United Kingdom, by petitions, by deputations, arguments, labored articles in the medical journals; by importuning of members, etc., etc., brought all the force it could command to prevent the passage of said bill. Their efforts resulted in a most humiliating failure. During the pending of a bill of like character in the Pennsylvania Legislature, two medical men from Philadelphia visited the Legislature, and by a statement before it completely snuffed out the whole affair.

Their compensation shows them to be held in low estimation by their fellow men. A gentleman of good practice in London, living at an expense of $5,000 per annum, will often furnish advice and medicine for 25 to 35 cents. Scores of highly competent medical men in London are struggling and will continue to struggle for years, scarcely able to keep the wolf from the door. It is simply impossible that a man in this condition should occupy a high social position. He could scarcely be considered other than a "pariah from society," which society makes a god of wealth, and bows with obsequious adoration before its idols of gold and of silver. Sometimes the long lane turns, if health and pluck endure. One of the most eminent men now in London scarcely earned his salt until he had passed his fortieth year. The case is different now. He is one of those who "weeds" his practice. A proper appreciation of the services of a doctor seems to be absent. The other day a "gentleman," —the word is used in its most flexible sense—drove up to a surgeon's door with liveried driver and lackey, entered the office with a flourish, and presented an injured joint to be examined. This was done, and advice given, when this apparent gentleman threw five shillings ($1.25) on the table. "Pardon me, sir," said the surgeon, "you have made a mistake. My fee is one guinea." ($5.00). "Oh," he replied, "I never give a doctor more than that." "Well," said the surgeon, "then I shall be compelled not to receive anything." "Just as you please," was the reply, and scraping up the five shillings he coolly walked out. The surgeon had no alternative, and he got nothing.

Generally speaking, a professional man called to attend one of the nobility scarcely receives courtesies above those extended to a common tradesman, which courtesies here are not much. If

the case is not urgent he must await the leisurely movements of his patron, and attend when asked. For this delay he receives no explanation or apology, nor, however kindly treated by this patron in his sick chamber, would the physician dare to recognize him first on the street. He might be the recipient of a most brutal snub from Sir Blue Blood. Occasionally one loses a little of his sympathy for the maltreated Medicus when he sees him obsequiously bowing and scraping and fawning to a titled patron, and at another time coldly, imperiously and unsympathizingly cutting up and snapping at, yelling at and browbeating some helpless, suffering, trembling out-door hospital patient. The contrast between these two pictures is as that between a California May and a Maine December. Charge this not all to the doctor himself, but let his "raising" have its full share.

All good practitioners have probably read in their boyhood the Sunday-school story in which an errand boy in a store was asked by his former fellow tradesmen, the boot-blacks, how he liked it now being bossed about. "Oh, very well," he replied, "I pass it off on the ashman."

To these peculiarities of English society aforementioned is this much due. "Our insular condition," of which the Englishman is so fond of speaking, meaning his water-belted isle, is carried out in the life of the medical man. He has no more than a half dozen friends, and these form his world. All above him he fears; all below him he despises. He is, in fact, as hard on others as others are on him, and his social ostracism is in many instances from this reason merited. It is the most difficult thing in the world to get a half dozen medical men on the witness stand to agree on one of the most salient of medical scientific facts. I may also cite an instance where a medical man went on the stand and swore that a brother of the same hospital staff did not know or detect consumption in its plainest form, a failure to do which would have been a disgrace to his newest "clark" or junior hospital walker. And this voluntarily.

And yet, let it be plainly understood, and remembered too, that there are no more patient, laborious, tender-hearted, thoroughly qualified men to be found anywhere than are many types of the London medical man.

Edward S. McKee, M.D.

London, 27th July, 1881.

ARTICLE XII.

LONDON, Aug. 9, 1881.

EDITORS CHICAGO MEDICAL JOURNAL AND EXAMINER:—Although the present Congress will scarcely constitute an "epoch in the history of medicine," as the more enthusiastic affirm, it has been the most notable, as well as the largest gathering of medical men that the world has ever seen. Everything has conduced to the success of the enterprise—the presence of numerous representative men in every department of medical art and science, the carefully-prepared programmes for sectional work, the admirably located and easily accessible rooms; while the bountiful hospitality of the London profession, displayed in elegant receptions, banquets, excursions, garden-parties, etc., as well as in private entertainments, afforded both stimulus and opportunity for individual discussions and arguments, from which the real advantage and benefit of such meetings accrue.

The most impressive scene was the opening meeting in St. James' Hall. The floor and gallery were filled with perhaps 3,000 medical men, among whom gray and bald heads predominated. The chair was occupied by one of the most eminent of English physicians—a worthy representative of the immortal name of Jenner. On the right of the chair lounged the good-natured, heavy-jawed, puffy-eyed heir to the throne of England, whose somewhat *blasé* mien presented a marked contrast to the manly features, erect figure and martial bearing of his illustrious brother-in-law and neighbor, the Crown Prince of Germany. Behind these sat a small, spare, rather effeminate looking man, whose mincing movements and careful toilet seemed curiously inappropriate for the famous Langenbeck, of Berlin. A striking contrast was a typical Englishman who sat near—a stout, compact figure, ruddy face, massive head, crowned with long silvery hair, keen gray eyes, thin compressed lips—agile, nervous Spencer Wells. Close by, a modest, rather shabby, insignificant looking individual, with the characteristic sandy complexion of the Scotch, was Keith. At a table on the left, an object of general attention, sat a wiry, restless figure, conspicuous by high cheek-bones,

a head tapering like a cocoanut, and a disposition to chatter constantly, the most genial scholar and scholarly genius of them all—Rudolf Virchow.

Further back, a tall, spare, muscular figure, distinguished among the gray heads by coal-black hair, with a rather austere, puritanical countenance (aside from a most unpuritanical blush at the end of the nose), and with a gravity of manner becoming his puritanical name—Jonathan Hutchinson. A massive, rather unwieldy form, with a fine head, sallow skin, and iron-gray hair and beard, arose in response to calls of Pasteur. A most impressive face—delicate features, high forehead, clear eyes and flowing white beard, added to a suavity of manner worthy of a princess' husband, distinguished Esmarch of Kiel. Near him sat a man whose name instantly rivets attention upon a face notable only for the immense expanse of brow. It seems incredible that this quiet, youthful looking individual (he does not seem forty-five) can be Volkmann, of Halle.

But I will not weary you with further enumeration of the hundred eminent men who occupied the same platform.

After a brief and sensible introduction by Sir William Jenner, delivered in a timid, clumsy way, the Prince of Wales read a few platitudes, and declared the Congress open. Then came a most eloquent practical address by the President, Sir James Paget, the delivery of which, in a beautifully clear, well-modulated voice, though marred by occasional awkward gestures, secured the close attention of the vast audience for three-quarters of an hour, after which the Congress adjourned for sectional work.

This work consisted, to an unpleasant degree, of twaddle by ambitious nonentities, who hoped to achieve immortality by appearance on the rostrum of the Congress, and who thereby wasted much valuable time in the reports of wonderful operations, in the description of wonderful apparatus, in the promulgation of wonderful views and theories. Yet several questions elicited real discussion, in which the leaders of medical and surgical practice put upon record their views of several still unsettled topics. It is flattering to our national pride, that we have produced not only the best horses, oarsmen and pedestrians of the year, but that the palm for advances in surgery has been almost unanimously

awarded to America. Lithotrity at a single sitting, involving often the use of a large catheter (29 or 30 Charrière) for evacuation, and the presence of instruments in the bladder for even two or three hours—in short, *Bigelow's* operation, received flattering testimonials from many, and marked approval from *all* speakers except Sir Henry Thompson. He objects to the use of the large catheter, which Bigelow insists is the essential feature of his operation, preferring to use a small lithotrite for the removal of the larger fragments. His friends are compelled to admit that, while this procedure may be safe and effectual in Sir Henry's practiced hand, it might be quite different in that of the average surgeon; while his enemies (and they are numerous in London) ascribe his opposition to personal jealousy of Bigelow. The latter's operation, therefore, while it may not effect, as the enthusiastic Telvan affirmed, a revolution in vesical surgery, is certainly the recognized and fashionable mode of lithotrity.

Emmett's operation for lacerated and everted cervix, although meeting far more vigorous opposition than Bigelow's, received the endorsement of all the representatives of the American gynæcological school, and of several among the English. Playfair, in particular, made a terse and witty speech narrating his conversion to Emmett's views, as based upon a case which he (P.) had "tinkered" with various caustics for several years, and which Emmet cured completely by a single operation. The operation will doubtless now receive at least a fair trial by the conservative English, few of whom have hitherto deigned to test it, as I was informed personally by Mr. Wells and Dr. Bantock.

The medical and pathological sections labored for two days to determine the relations of renal disease to general vascular changes. Gull and Sutton attempted to force their "arterio-capillary fibrosis" heresy down the reluctant throats of the Germans; Geo. Johnson harped on muscular hypertrophy, as usual; Rosenstein gave an admirable *résumé* of the German views as to the pathology of the kidney; but when Grainger Stewart hove in sight with his interminable catalogue of imaginary "Bright's diseases," the writer fled. What the issue of the conflict was, I have not heard; but as the combatants were chiefly Gaels and Teutons, it is safe to assume that no conclusion was reached.

The sharpest discussion was that upon antiseptic surgery. Spencer Wells said that he had not used a drainage tube in ovariotomy during the last three years. (I have heard him say that he meditates abandoning the spray). Lawson Tait referred to thirty-six cases of unusual intraperitoneal operations—removal of gall-stone, opening of hydatids of liver, cysts of kidney, abscesses of pelvis, section for Fallopian pregnancy, etc. But one patient died. In only a few cases had he attempted to use Listerian details, which he soon abandoned as cumbrous and impracticable; sometimes positively injurious.

Keith stated that after losing two ovariotomy patients by carbolic acid poisoning, he had renounced the spray, and had since had sixty-five cases without a death.

Savory, while complimenting Lister on the improvement in the sanitary condition of King's College Hospital, referred to the fact that antiseptic surgery, in Lister's own hands, could not show results as good as those by the old plan at St. Bartholomew's.

On the other hand, Esmarch, Volkmann and Czerny related wonderful improvement in the statistics of their operations since they had adopted Lister's method. In the Samaritan Hospital here Thornton had twenty-one cases of ovariotomy, with one death—all carefully Listered; his colleague, Bantock, shows a similar record, except that he will not allow a drop of carbolic acid or other antiseptic to be used during an operation—employing simple water dressing.

But the crowning blow for antiseptic surgery was dealt by Lister himself. Attempting to reply to Keith, he said that he had always advised against the use of the spray in intra-peritoneal operations! That inasmuch as contagion occurred through fingers and instruments as much as by the air, the spray might perhaps be laid aside without much danger; that although he was not ready to take the step, he *might be* in *another year!* It was an aggravated case of "crawfishing." Esmarch, Volkmann & Co. looked decidedly disconcerted. It looks as if the bacteria superstition was about exploded even in Lister's own mind, and that he is simply "prospecting" for a good excuse, under cover of which he may extinguish his absurd and dangerous spray, and base the claims of antiseptic surgery where they belong.

I can not conclude without recording the brilliant success

achieved by Dr. Billings' wise and witty paper on Medical Literature. The close attention, signs of frequent approval and appreciation, and generous applause at the close, were handsomely crowned by Sir James Paget's remark from the chair, that if the Congress could show no other evidence of usefulness than the production of that paper, its existence would still be abundantly justified.

W. T. BELFIELD.

ASPIRATION IN DISTENDED GALL-BLADDER.—The time has come when the internal administration of drugs in cases of distention of the biliary passages should not be exclusively relied upon. Surgical interference should be practiced much more frequently than has been done. The advantages which aspiration possesses over other means of emptying the dilated gall-bladder, may be thus formulated:

I. The operation may be performed with *safety*, without taking particular precautions in uniting the walls of the gall-bladder with those of the abdomen.

II. The operation can therefore be done as soon as the diagnosis of the dilated gall-bladder has been made, if, from its size, there seems to be danger of rupture; or if the patient suffers much pain. Aside from these conditions, when aspiration should be resorted to without hesitation, the question presents itself whether it would not be good practice to evacuate the contents of a distended gall-bladder under all circumstances, simply to remove the superfluous bile, which, being cut off from its natural destination, is bound to be reabsorbed by the lymphatics, carried back into the circulation and produce, to a greater or lesser degree, a condition which is generally known as "cholæmia."

III. A very fine trocar, such as would be of not much value in case of simple puncture, can be employed, and by means of suction even a tenacious fluid can be removed from the gall-bladder.

IV. The insertion of a trocar or an aspirating needle is almost a painless procedure.

V. In cases of doubt as to the presence of gall-stones, a flexible probe can be passed through the canula and used as a sound.

VI. Aspiration being a safe and painless operation, it can be employed for the purpose of aiding diagnosis.—*Kretzschmar.*

Domestic Correspondence.

ARTICLE XIII.

MESSRS. EDITORS:—In the August number of the CHICAGO MEDICAL JOURNAL AND EXAMINER, page 178, is published the history of a case of "Extra Uterine Fœtation." As published by Dr. Rozencrans, the case is a very unusual one, and may fairly form the subject of criticism. Permit me to make a few remarks on it.

"Examination of this woman satisfied me that she was a subject for operation; that it would not admit of delay," etc. Yet the "operation" was postponed twenty-three days, and the woman, meantime, brought twenty miles in a wagon. This done, he then, and not till then, makes a thorough examination. "As there was evidently a considerable quantity of fluid in the abdominal cavity," he performed paracentesis abdominis one inch below the navel, drawing off two gallons of dark fluid; he then could feel a "solid substance" in the abdomen. Before making a thorough examination—before tapping—the reporter concluded "she was a subject for an operation," and had the woman removed a distance of twenty miles in a wagon. The data for coming to that conclusion, and for diagnosing an extra-uterine fœtation, are not vouchsafed us, as they might have been. Cases of this kind are obscure enough in their symptoms and signs, and we need further light on the method of diagnosis. Was not the tapping a most dangerous and unwarrantable proceeding? for he tells us, "after cutting through the walls of the abdomen, bringing to view the cyst . . . I . . . at once cut open the cyst," from which flowed the "same character fluid" as had been removed by the trocar. This proves that it was the "cyst" he tapped; that the

fluid was not in the abdominal cavity. Was there no peritoneum met "after cutting through the walls of the abdomen," and what was its condition? "I then tore away the placental mass, and proceeded to the removal of the sac." Removed the placenta! Is this the method of dealing with an extra-uterine placenta recommended by Barker, Thomas, Keith, or Spencer Wells? We think not. Where did the placenta grow? Where was the "sac?" and what were its adhesions? What was the condition of the fœtus? He seems to have cut and torn as fearlessly as he would on a cadaver. Why was the right ovary removed?

"To obviate internal bleeding," he, after closing the wound, "placed two bricks on the abdomen for pressure" for half an hour. Were the bricks put on to compress the abdominal aorta? if so, they cannot do it; and to check bleeding in the pelvis they are equally ineffectual. Why close the wound until all bleeding was arrested? From the rough, yes, crude, manner of operating, no one can feel surprise at being told there was "for many days a copious discharge of putrid matter" through the drainage tube, notwithstanding the use of the carbolic spray during the operation. "I applied pounded ice constantly for the space of three weeks to the abdomen." Is it possible for any human being to endure such cold *so long* and live? What was the need of it? How was the peritoneum, of which we hear nothing from beginning to end, dealt with in closing the wound?

Was it absolutely necessary to practice catheterism for a month? Were opiates used? "The temperature of her body remained quite normal," yet she had a pulse of 100 in the morning and of 120 in the afternoon, for about three weeks. What is the explanation of this high pulse-rate? Was the temperature accurately measured two or three times daily by the clinical thermometer? With a "copious discharge of putrid matter" for many days, catheterism for a week, a pulse up to 120 daily for three weeks, and ice to the abdomen constantly for the same time, it does look as though there had been, after all, some peritonitis. Such persistent and prolonged use of ice should be enough to deprive any peritoneum of sensibility. Hence the patient would not complain of pain. The woman was seventy-eight days—a very long time—recovering. What was the cause of this? "I am

not aware that a similar operation was ever before performed in this State." We hope not, nor in any other State.

Such is a calm and fair criticism of this case, as reported. What woman, subjected to such conditions, could live after them? When scrutinized, the narrative looks, to me, improbable on the face of it, and beyond the name of Dr. Rozencrans, bears no evidence of authenticity. Is it meant for a rude hoax? Did the operator live in Southern Texas at the time? Did he go from Elgin to Texas to operate? or is he lately come from the part of Texas mentioned to reside in Elgin, Ill.?

THE METHOD OF REMOVING FOREIGN BODIES ENGAGED AT THE RIMA GLOTTIDIS.—M. Krishaber (France Med. 16–19, Dec. '80) relates four cases in which the foreign body rested on the superior surface of the vocal cords. In two cases a 50 centime piece was retained by the laryngeal ventricles in a horizontal position. One of these patients was placed upon his abdomen in recumbent posture, the index finger of the left hand passed as far as the edge of the epiglottis, and a pair of laryngeal forceps introduced, but it was impossible to seize the piece, which slipped from the grasp of the instrument. The patient was then placed in front of the operator, a pair of closed laryngeal forceps introduced below the glottic orifice, then opened, being careful to exercise pressure from below, upwards. The piece was dislodged and accidentally swallowed by the patient.

In the second case the piece was pushed out by means of a sound introduced through the tracheal opening made to save life, which was threatened by œdema. A bone was removed from the glottic orifice in the third case by means of forceps.

In the last of the four cases a piece of copper was extracted by the same procedure which failed in the first case mentioned.

Krishaber advises except in the case of infants in whom the larynx is easily reached by the finger of the operator, the performance of tracheotomy, the tamponning of the trachea and the extraction of the foreign body *per vias naturales*.

Editorial.

OUR CITY MORTALITY.—From the statistical tables furnished by the Health Department of this city, it appears that the gross mortality for July was 1,786, and for August 1,730. These figures not only indicate that the mortality for these months is as usual double that of any other two months of the year, but that their mortality is decidedly above the ordinary ratio for the same months in previous years. The increased mortality is as usual mostly from bowel affections in young children. But aside from this there were, in July, deaths from typhoid fever twenty-three, scarlet fever nineteen, diphtheria twenty-two, cerebro-spinal fever twenty-six, cerebral meningitis fifty, and small-pox fifty; and in August, from typhoid fever ninety-eight, scarlet fever fourteen, diphtheria forty-nine, cerebro-spinal fever thirty, cerebral meningitis forty-five, and small-pox 116. These figures, especially so far as they relate to typhoid fever, cerebro-spinal fever, and small-pox are extraordinary. Is the fault in the peculiarities of the season or in the sanitary condition of the city, or in both combined?

INTERNATIONAL MEDICAL CONGRESS.—So far as we have been able to learn, the recent international medical meeting in London was a very satisfactory one, both in regard to the number of delegates in attendance and the quantity and quality of work done. The arrangements appear to have been well made for transacting the proper business of meeting, while the hospitalities extended were abundant and cordial, and the number of those present from the various nations on both sides of the Atlantic was sufficient to give it a more truly international character than any

of the congresses that had preceded it. We have been on the lookout for some items concerning the important work done there by the numerous delegates representing our own city, but our looking has thus far been in vain. We must be content, therefore, until they come home and report for themselves.

CHICAGO DOCTORS.—Dr. A. H. Foster has furnished us with the following interesting statistics concerning the number of physicians and surgeons in this city, recognized as regular and in good standing by registration in the *Chicago Medical Register* for 1881. The whole number of names in the said register is 452. Of these, 234 are graduates from the regular medical colleges in Chicago; namely, eighteen from the Woman's Hospital Medical College, eighty-six from the Chicago Medical College, and 130 from the Rush Medical College. This would give one regular physician for every 1,112 inhabitants. Lest there should be some benevolent individuals who might fear this ratio indicated an inadequate supply of doctors in our city, we will state that the *Chicago City Directory* (Lakeside) for 1881, has the names of 892 persons claiming the title of "Doctor." This would indicate a doctor of some sort for every 563 inhabitants.

TRI-STATE MEDICAL SOCIETY.—This society, composed of members of the profession in Indiana, Illinois, and Kentucky, will hold its next annual meeting in the city of St. Louis, Missouri, commencing on the morning of the 25th of October, 1881, and continuing three days. We hope our brethren on the west bank of the Mississippi will not be frightened by the invasion of their territory, and that the members of the society after enjoying an unusual "good time," will all succeed in finding their way back to their respective States in safety.

IN view of the opinions lately expressed by eminent oculists, that the reading of German text is injurious to the eyes, the Bernese Government has resolved as much as possible to discourage its use, and all their official announcements and reports will henceforth be printed exclusively in Roman characters.

The Death of The President.

As we go to press, the City of Chicago is putting on the garb of mourning for the grievous loss it has sustained in the death of the Chief Magistrate of the Nation, JAMES A. GARFIELD. In common with the people of this and other countries, we feel keenly the grief which is awakened not only by the atrocious character of the wicked crime which has led to this result, nor yet only by the lofty position of the victim of the assassin's blow, but by the personal character of the noble martyr. His was a scholarly, an intellectual life, which appeals to the sympathies of all scholarly men, the world over. His was a gentle, home-loving and home-honoring life, which the heads of the families in this broad land were prompt to revere. His was a forgiving, unmurmuring and charitable spirit, such as that whose influence was evident in the counsels of the nation years after the body of Abraham Lincoln was laid in the dust. We have tears for his loss, gratitude for his example, reverence for the man who went down to the valley of the shadow of death without a reproach upon his lips for his murderer, and loyal honor for the gentleman who by this accident, becomes in his place the President of these United States.

One word as to the medical and surgical treatment of the case. Manifestly, until the professional gentlemen who have been in charge of it, shall have made up their record and presented it in full to their peers in medicine, it would

be a discourtesy to either criticise or condemn. In the high position of Drs. Agnew and Hamilton, the profession at large have a guarantee that what was done was well done and worthily. The results of the autopsy have shown that no knowledge as to the course exactly traversed by the ball, was had by those who treated the wound. This is exactly and precisely the position taken from the first by Drs. Hamilton and Agnew. When questioned and interviewed, these gentlemen have repeatedly and properly asserted that it was impossible to state what course the ball had traversed. The newspapers, with characteristic savagery, are even at this moment assuming that a grave error had been committed. It will be remembered that at the time of the interesting and valuable investigations made by Dr. Weisse, of New York, some account of which has appeared in these pages, Dr. Hamilton was asked his opinion of Dr. Weisse's theory. The reply was, that he (Dr. Hamilton) was "interested," in the same. He could not, however, state that the course of the ball had been decided by the experiments, because it was his opinion that the former "had not been determined." As for the treatment of the suppurating track which many took to be the course of the ball, that surely should have been the same in either event. It is no small tribute to the skill and wise judgment of the surgeons of the late president, that they wisely refrained, in the face of considerable popular clamor, to seek for the missile to an extent which might have proved mischievous, and, as the result shows, would have proved futile; the fact being that now when the case is ended and its hopeless features evident to all, there can be but praise for the skill which ministered to the invalid for so long a period without once placing his life or comfort in jeopardy.

Reviews and Book Notices.

ARTICLE XIV.—ON THE ANTAGONISM BETWEEN MEDICINES AND BETWEEN REMEDIES AND DISEASES. Being the Cartwright Lectures for the year 1880. By Roberts Bartholow, M.A., M.D., LL.D., Professor of Materia Medica and General Therapeutics in the Jefferson Medical College of Philadelphia; Author of a "Treatise on Materia Medica and Therapeutics," of a "Treatise on the Practice of Medicine," etc. 8vo, cloth, pp. 122. $1.25. New York: D. Appleton & Co. 1881. Chicago: Jansen, McClurg & Co.

It has long been a received principle in medicine that the derangements caused by disease may be opposed and thereby made to disappear by other derangements caused by medicinal substances. Physiological antagonism means an opposition of action of poisonous medicinal agents, in that the effect of the one may be exactly counterbalanced by the effects of the other. But "such completeness of opposing action is as rare as exact similitude in remedies acting in the same way." The Hippocratic aphorism, "in general, diseases are cured by their contraries," remains true. The author then explains the doctrine of similars. "A little consideration must, I think, tend to the conclusion that, when a remedy acts in a similar manner to a disease, there must be an antagonism between the force of the remedy and the momentum acquired by the disease. If the actions were the same, the result of the combined impression would be an increase of the disturbance."

He establishes thus his theory on the basis of a general principle propounded by Herbert Spencer, that the result of the various natural forces acting together is rhythm in action. Bar-

tholow had read Spencer before writing the following: "I am not prepared to say, though I would remark that facts are not an invention of the mind, but have their reality in nature, only awaiting our investigation." Bartholow says: "In the medulla oblongata is situated a center of extensive reflex sensibility, and above it is an inhibitory center of reflex movements." The inference is that antagonistic remedies act on each of these respectively. "In the cardiac and respiratory mechanism we have admirable illustration of opposing forces producing order and rhythm. The movements of the vessels are regulated by a vasomotor center in the medulla," etc.

That rhythm is the law, however, must not be considered a truism, for, as Bartholow intimates a few lines further, whenever forces interfere in equal proportions they cease to be perceptible, so that only the rhythmical action strikes our senses and thus leads us to think that forces in action are always rhythmical. The perusal of the first lecture will repay any anxious student of philosophy and nature, and leave him convinced that the treatment of disease mostly consists in destroying a natural force (the disease) by another natural force (the remedy or medicinal agent.) The investigation of recorded cases of poisoning by opium, in which belladonna was given as an antidote, is masterly and does honor to the author. A practical point derived from the antagonism of morphia and atropia is their efficiency in preventing any bad result from either drug when given together, while their beneficial effect is increased.

The principle of physiological antagonism is destined to cause an important reform in therapeutics, and any one perusing the book under consideration will get many hints which may prove very valuable in practice; although some of them are startling ideas, they are the more reassuring since they come from Bartholow, and have been, most of them, the result of his careful investigations. Among these, the danger of administering chloral hydrate together with bromide of potassium, and chloral and morphine in full doses, is mentioned. The author claims, however, that an hypodermic injection of morphia, before administering an anæsthetic, is a valuable precaution. Strychnia in paralysis and in phthisis, belladonna in phthisis, and strychnia with

ergot in hæmorrhages are all highly recommended. But although comparatively small, this treatise on antagonism contains so many items of information that one could not review more than a few of them, and we must state that a large part of it is concerned with physiological antidotes to vegetable poisons,—a series of facts which every physician must take knowledge of, as it would be considered a criminal neglect not to know the value of belladonna in opium poisoning, etc. It is evident that every student and practitioner of medicine needs this book, and that so much knowledge in so small a compass is very unusual in medicine. H. D. V.

ARTICLE XV.—ON THE BILE, JAUNDICE, AND BILIOUS DISEASES. By J. Wickham Legg, F.R.C.P., Lond.; Assistant Physician to St. Bartholomew's Hospital, and Lecturer on Pathological Anatomy in the Medical School. 8vo, cloth, pp. 720. New York: D. Appleton & Co. 1880.

It will be seen from the title that this is a contribution to physiology and to chemistry, as well as to the practice of medicine, though given mostly to the latter. The subject is a vast one; the author had to follow untrodden paths at times, and the whole necessitated a prodigious amount of experiments. As the fanatical and ignorant act against vivisection had just been passed in the British Parliament, Dr. Legg addressed himself to the French Brotherhood and conducted many of his investigations at the Lyons laboratory, one of the best furnished in Europe. The author expresses himself very bitterly on this subject, calls the agitation a senseless outcry, and adds: "For whatever imperfections there may be in the method followed, the reader must blame those Manichæan teachings, which, harmful as they are, are almost as old as the human race itself, and the germs of which seem to exist everywhere, ready to burst into a fresh life, even amongst those who claim to be most othordox and sincere in their morals and belief."

The author's experiments with the bile of various animals resulted in the fact that its components vary in quantity and quality according to the species. The bile of salt-water fish—the cod, and the turbot, for instance—contains chiefly taurocholate of

potash, while the bile of fresh-water fish held much more soda. As a whole these experiments do not seem very satisfactory, and they will not benefit students who need better sifted material. In "Summary of Knowledge as to the Physiology of the Bile," we meet with the statement that "it is uncertain if the liver be a mere filter, or if it secrete the bile itself, although the general belief of physiologists is in favor of the latter." "Whether the breaking down of the tissues, or the splitting up of the peptones from the food furnish the bile acids is not known." "During fasting the amount of bile is much decreased. Purging causes a decrease in amount of bile secreted. Vomiting is followed by an increased expulsion of bile; so also is muscular exercise."

Chapters X to XIX comprise probably the best treatise ever written on the subject of jaundice. Although this is more a symptom than a disease in itself, still any reader will admit after perusing these 188 pages that the subject is better treated in this manner, and rendered much more intelligible to students and even to practitioners.

The following remedies are valuable in the treatment of jaundice: Ferruginous, citrates, acetates of sodium and potassium, mineral waters, lemon juice, acids, both mineral and vegetable.

Chapters XIX to XXIII are devoted to acute yellow atrophy, or icterus gravis. As to its treatment, the author recommends first, a smart mercurial purge, followed by sulphate of magnesia, then large doses of quinine, with an admixture of the mineral acids; locally, a large, warm linseed poultice over the liver. Jaundice after poisoning by phosphorus, copper, mercury, arsenic, antimony, etc., is also described; and so the jaundice which often complicates fevers.

Chapter XXIX and last, treats of bilious diseases, in fourteen pages. This is less than the title of the book suggested. Most fevers come under that head in Hippocrates and Galen's writings. According to Stoll they are troubles of digestion, as diarrhœa, etc. Also, other writers bring under this head a decreased bile secretion.

"Here in England, a bilious attack means vomiting, with headache. If the word bilious means a gastric catarrh, there are still objections to its use."

This book, so far as the matters treated go, will remain a valuable contribution to the study of bile and the treatment of jaundice, and we would recommend every practitioner to get a copy, even if it were only for making references when treating icterus neonatorum, or acute yellow atrophy. H. D. V.

ARTICLE XVI.—MEDICAL ELECTRICITY: A Practical Treatise on the Applications of Electricity to Medicine and Surgery. By Roberts Bartholow, A.M., M.D., LL.D., Professor of Materia Medica and General Therapeutics in the Jefferson Medical College of Philadelphia, etc. Author of "A Practical Treatise on Materia Medica and Therapeutics," and of "A Treatise on the Practice of Medicine," etc. With ninety-six illustrations. 8vo, cloth, pp. 262. $2.50. Philadelphia: Henry C. Lea's Son & Co., 1881. Chicago: Jansen, McClurg & Co.

This comes up to the standard of the author's other works, already so well known. Dr. Bartholow represents rationalism in medicine, and every line dropped from his pen is the expression of sound theories and practical facts. One should not expect to find electricity in medicine so highly commended as in Beard and Rockwell's large work, for this author is not a specialist, and would not perhaps deny the following statement of a contemporary: "Electricity has not as yet attained a very wide range of usefulness in medicine or surgery. The cost, the inconvenience, and the lack of knowledge have had something to do with this. And besides, the intrinsic value of the agent is undoubtedly less than was at first supposed and hoped." Yet electricity in the hands of the author has been very serviceable in the treatment of paralysis, rheumatism, many nervous diseases, and in constitutional diseases also; electricity at times acting as a tonic, and also as an alterative.

This book is well printed, the type is large and clear, and the contents are about all that a busy practitioner has time to peruse. It is also well adapted to students of medicine, and will sufficiently supplement their knowledge of electro-physics, of which so few of them know anything, resembling their preceptors in this. Although some enthusiastic minds once cherished the idea that

electricity was the source of all other forces, that it was the soul of nature, and consequently a promoter of life, these theories have faded away, and medical electricity received a great stroke at the hand of that writer who lately proved that it was mainly a paralyzing agent. However, electricity remains one of the surest ways to influence the mind, and thus exercise a beneficial effect on the body. H. D. V.

ARTICLE XVII.—HERNIA, STRANGULATED AND REDUCIBLE. With Cure by Subcutaneous Injections, together with Suggested and Improved Methods for Kelotomy. Also an Appendix, giving a short account of various Surgical Instruments. By Joseph H. Warren, M.D., Member of American Medical Association, etc. 8vo, cloth, pp. 280. $3.00. With illustrations. Boston: Charles N. Thomas, 215 Tremont street. London: Sampson, Low, Marston, Searle, and Rivington. 1881.

This is certainly a *multum in parvo*, and that little consists in the author's operation, which is detailed in seventy-three pages, and is, as the profession is aware, a very promising treatment. A few quotations will convey the author's idea. A special syringe, invented by Dr. Warren, should be used, as no other hypodermic needle than his will answer the purpose. The legs of the patient being drawn up and the muscles relaxed, the hernia should be returned, then, "we pass the left middle finger up the spermatic canal until we come to the inguinal ring, and by slightly raising the end of the middle finger as above mentioned, the same is felt by the fore finger, which also helps us to indicate the exact point, and guide to insert the point of the instrument. Having ascertained that the ring is well open, and free from attachments or adhesions to the returned sac, we begin to insert the needle at the lower portion of the ring, where we feel its edges through the abdominal parietes. The needle should always enter this lower portion of the ring, as in passing obliquely upwards and backwards it is less likely to wound either column of the internal ring. Great care should be taken in inserting it through the integuments and superficial fascia, so as not to wound

the external pillar, but to enter the canal at once. The needle then should never be passed in a perpendicular direction, as there is thus danger of wounding the spermatic cord, but it should receive the necessary obliquity as soon as we feel that it has passed through the integuments. We can diagnose the position of the needle when first entering, by passing the left fore and little finger up with the invaginated scrotum upon it. When we have passed the needle though the integuments, we begin to open the valve and slowly push the needle in the direction already indicated. As the needle is thus inserted, it revolves and injects the fluid in sufficient quantities to cover well the external and internal rings. The needle is now slowly withdrawn, still injecting fluid in its backward motion. As soon as the needle is withdrawn pressure is made with the end of the fingers over the wound and rings for five or ten minutes, until the smarting and throbbing pain subsides."

A compress is now applied over the inguinal canal, and should be kept wet with cold water. The patient should stay in bed for a few days, and opiates should be administered during that time. Dr. Warren claims that all cases do well, and that was also the statement of the late Dr. Heaton, who used an infusion of white oak bark for an injecting fluid and a blunt hypodermic needle. Any one desiring to employ this treatment of hernia should read Dr. Warren's book and procure his instruments.

The reviewer believes in the correctness of the author's statements, yet would add that notwithstanding the wide advertisement which this method had in Europe as well as in America, he has not come across any statement in the medical press of France, England, Canada, nor the United States, to corroborate these wonderful cures. Of course this method of treatment is so new that positive results could not be well ascertained, although the seemingly harmlessness of the operation should open a wide field to enterprising surgeons, and it is time that it should be rescued from the hands of quacks. H. D. V.

Summary.

VACCINATING FOR ANTHRAX ON A LARGE SCALE AND SUCCESSFULLY. By PASTEUR.

In April last, the Agricultural Society of Melun proposed to me, through its president, the Baron de la Rochette, to settle, by decisive experiments, the results announced by me in the Academy. I consented, and April 28 the following agreement was made:

1. The Agricultural Society of Melun shall place sixty sheep at the disposal of M. Pasteur.

2. Ten of these sheep shall receive no treatment.

3. Twenty-five of these sheep shall be vaccinated twice, with an interval of twelve days, two unequally attenuated viruses being used for that purpose.

4. These twenty-five sheep, as well as the twenty-five remaining, shall be inoculated with very virulent anthrax, after another interval of twelve or fifteen days.

The twenty-five unvaccinated sheep shall all die; the twenty-five vaccinated sheep shall resist, and shall be compared ultimately with the ten sheep kept in reservation, in order to show that the vaccinations do not prevent the animals from returning to a normal state.

5. After this general inoculation of very virulent virus to the two sets of twenty-five sheep, vaccinated and not vaccinated, these fifty sheep shall remain together in the stables; one of the series being distinguished by punching the ear of the twenty-five vaccinated sheep.

6. All those sheep which anthrax will kill shall be separately buried in an inclosed space.

7. In May, 1882, twenty-five fresh sheep, never used before in experimenting, shall graze in this space, in order to show that fresh sheep will spontaneously contract anthrax from the germs of the disease which earth-worms shall have brought from the cadavers to the surface of the ground.

8. Twenty-five more fresh sheep shall graze around this inclosed space, a few meters off, where no cadaver has ever been buried, in order to show that none of them shall die from anthrax.

Addition to the preceding convention programme. — The President of the Agricultural Society of Melun having manifested a desire to have these experiments include cows, I gave my consent, remarking, however, that until now, the experimental vaccination of cows had not been carried to such an extent as that of sheep, and that consequently the case might be that such experiments might not be so encouraging as those on sheep. In any case, I expressed my gratitude to that society for placing ten cows at my disposal, in order that six might be vaccinated and four left alone, and that after this vaccination the ten cows should be inoculated at the same time as the fifty sheep with most virulent virus. I affirmed, on one hand, that the six cows previously vaccinated would not be sick, while the four unvaccinated cows should perish partly or in a whole, or at least would be very sick.

I own such a programme showed a boldness in prophesying that only a brilliant success could warrant. The experiments were begun May 5, in the commune of Pouilly-le-Fort, near Melun, on a farm belonging to M. Rossignol.

In order to satisfy the wishes of the Agricultural Society which started the experiments, it was agreed to replace two sheep by two goats, and, as no condition of age or race had been proposed, it happened that the fifty-eight sheep differed in age, sex and race. Among the ten beeves were eight cows, an ox and a bull.

On May 5, 1881, were inoculated by hypodermic injection twenty-four sheep, a goat, and six cows, five drops of an attenuated anthrax virus being injected into each animal. May 17 we re-inoculated these twenty-four sheep, the goat, and the six cows with another attenuated virus more powerful than the first.

May 31, we proceeded to vaccinate with the most virulent

virus, in order to test the efficacy of former vaccinations. In order to do that, we inoculated on one hand the thirty-one beasts just spoken of, on the other, twenty five sheep, a goat, and four cows—none of the latter having been previously vaccinated.

The most virulent virus, used May 31, was one regenerated from the germinal corpuscles of the anthrax bacteria, which I had kept in my laboratory ever since March 21, 1877. In order to insure more comparative experiments, a vaccinated animal and one not vaccinated were alternately inoculated. June 2, the results were wonderful. The twenty-four sheep and the goat which had been vaccinated, together with the six cows, were all in good health; on the contrary, twenty-one sheep and the goat, which had not been vaccinated, had already died of anthrax. The three other sheep of this series died the same day.

The non-vaccinated cows were not dead, being less liable to die from the disease than sheep; but all had large swellings about the point of inoculation. Their temperature went up three degrees. The vaccinated cows had no rise in temperature, no tumor, and no inconvenience; and this shows the vaccination of cows to have been as successful as that of sheep.

ST. ANN ASYLUM. Service of PROF. BALL.

Syphilitic Insanity.—The influence of diatheses on the evolution of mental affections is now an uncontested fact. Rheumatism and gout both give rise to real insanities, which we studied in our preceding lessons under the names of rheumatic and gouty insanity. Syphilis, as the other diatheses, can also originate insanity. You know that syphilis, after it has attacked the skin and the mucous membranes, the bones, the muscles and the cellular tissue, finally takes to the viscera, and causes well-known lesions in the various parts of the nervous system. Presently, cerebral syphilis is no longer contested, and it was masterly investigated by Prof. Fournier. Anteriorly to this writer, Dagonet had sustained and proved the existence of syphilitic insanity, of which he had met a few cases at St. Ann Asylum. In Germany, syphilitic psychoses have been investigated. We shall in turn resume our actual knowledge of that affection.

And first, to what lesion shall we refer this special form of insanity, caused by syphilis? To cerebral lesions? They are so numerous, indeed, that one is only embarrassed by the choice. To an alteration of the blood? or to an unknown cause? This point has not yet been settled.

In a syphilitic patient, at what time in the course of the disease, at what age of the diathesis, says Fournier, do the cerebral manifestations appear? . . . These may come sooner or later. Although in some cases they are manifested a few months only after the original chancre, in others they may not appear till twenty or thirty years later. The medium is about the tenth year, but the exceptions are numerous.

Why should syphilis attack the brain? Here let us note that it most generally occurs in persons whose general health has much suffered, and who present that state called venereal cachexia, or syphilitic chlorosis. Beside the constitution, there may be exciting causes; for instance, the various venereal excitements, excesses of coition following long continence, which have so great an influence over mental manifestations, which are thereby perverted and disturbed in people predisposed. Add to this a powerful and preponderant cause, alcoholic excesses. Take into consideration fatigues and excesses of intellectual work, which have made the brain a *locus minoris resistantiæ;* this is an influence easily noticed among writers, lawyers, politicians, who are much more often attacked by syphilis than the lower classes, among which it is rather rare. Finally, a last cause, which no one could deny, traumatisms, strokes, and shocks to the head.

This being admitted, let us make another query before forming a judgment. Which kind of syphilis most likely originates cerebral lesions? You are aware, gentlemen, that there are benign, common and malignant syphilis; these are specially manifested by the rapidity of their course, and cutaneous lesion. Cerebral lesions are mostly excited by syphilis of medium intensity; yet we should fear the mild forms, sometimes very insidious, and not forget that there are abortive forms in which secondary manifestations may not appear. Let us now come to the symptoms of syphilitic insanity. We will overlook the other manifestations of cerebral lesions: paralyses, aphasia, amnesia, etc.,

which sometimes precede or accompany the disease under consideration. The first intellectual accident may suddenly appear. According to Fournier, insanity is often introduced by a notable failing of the intellect; then comes the syphilitic delirium, which may present itself under three forms:

1. Syphilitic dementia.
2. Syphilitic mania or insanity.
3. General syphilitic pseudo-paralysis (Fournier).

1. *Syphilitic dementia.*—The sufferer from syphilis, after a certain period, changes his moods. Some disturbance of the intellect, and peculiarities of his behavior are noticed. One who was before polite, active, bright, witty, becomes by degrees morose, impolite, dull, numb, reticent, speechless; intellectual labor is difficult, work painful, tiresome, even unbearable, in the discharge of the common duties of life. Fournier considers loss of memory a remarkable phenomenon. For my part, gentlemen, that is a trivial phenomenon; for of the various faculties of the brain, memory is the most delicate and fallible. The judgment, on the contrary, often remains right. Syphilitic amnesia may be slow and continuous, or arise suddenly. The loss of memory is sometimes almost complete, and the patient, as do some old men, may have none but youthful recollections left, recent events being forgotten; some patients even forgetting their children. Others have forgotten the simplest rules in arithmetic; others a foreign language; others at last have lost their notion of time and distance. Add to this an absolute incoherence of ideas and actions. The patient now reaches a complete dementia; being mean, filthy, disgusting and coarse. Besides a defective moral sense, and peculiar habits, the patient is so irritable as to get mad at anything, though his anger is short. To resume, this is a premature senility of mind, differing from common old age in this, that it can be cured.

2. *Syphilitic mania or insanity.*—Syphilitic delirium, as well as dementia, may appear with or without prodroma. In some there is a stage of excitement, characterized by a feverish activity of the individual, who is restless, speaks much, and forms a quantity of projects. This soon passes into frenzy and insanity; this frenzy appears under various forms. Sometimes it is a

properly syphilitic mania, in which the patient, suffering from a true muscular delirium, breaks everything which comes in his way; his words are incoherent, there is complete insomnia, as in the excitement stage of general paralysis; this might lead into some error of diagnosis, but these acute symptoms are more marked in true mania, while there is no other differential symptom. Besides that form, lypemania, with a tendency to suicide, may appear in some subjects; 2, then hypochondria in all its forms, syphilophobia especially; 3, the delirium of persecution, though vague and confused; 4, last, and this is important, there is sometimes an impulsive delirium, especially in syphilitic epileptics. A delirium of hallucination has also been described.

3. *General syphilitic paralysis.*—You are aware, gentlemen, that some authors believe syphilis the chief cause of general paralysis, while others think it has no influence in this respect. The truth lies between the two, and Fournier admits the existence of a peculiar form of syphilis of the brain which gives rise to a general pseudo-paralysis. This is manifested by an incoherence in the intellect, both in words and writings. An ambitious frenzy is rare; patients being generally dissatisfied with themselves, and they sometimes have excesses of low melancholy, as in general paralysis. Their speech is often impeded, and they stammer from paralysis, not from a muscular contraction, as is the case in general paralysis. Besides, in syphilis there is no tremulousness of the fibrillæ of the tongue, of the upper lip, nor of the facial muscles, but there is sometimes partial paralysis, especially in one eye.

A true diagnosis can be made through complications and concomitant symptoms mainly; as cephalalgia, alopecia, gummata; but especially through partial paralyses, an important indication of syphilis of the brain.

"Strabismus, it has been said, suggests syphilis." Finally, an exhaustive and severe inquest should be made on the body of the patient, his history looked into, and not only should his family be questioned, but especially his comrades.

Whenever the patient comes under timely treatment, the prognosis is good. Hence it is necessary to act promptly and energetically. At first the mild treatment: potassium iodide, 5

grains at first, and inunctions of mercury, without any fear of any bad results from such a course (Fournier). Alternate medication is better tolerated. In case mercury could not be borne, chloride of gold may be beneficial. Finally, one must not forget that there are bad cases in which, in spite of any treatment, syphilis results in death.

MANAGEMENT OF LABOR, WITH THE PATIENT IN THE ENGLISH POSITION. By Dr. Rendu.

Labor on the side, also called the English position, because women are delivered in that way in England, is also the manner of delivery in most of the lying-in hospitals of Switzerland and Germany, but only in primiparæ. Multiparæ are delivered on their back, in France, because the elasticity of the external genital organs protects them more against lacerations of the perineum. In fact, the chief advantage of that method is to render much less frequent lacerations of the perineum in primiparæ. One can thus survey very well its progressive distension, and that of the vulva, and consequently one can appreciate better the support which the parts require; beside, the accoucheur is in a better position to give a more efficient support.

It is no exaggeration when I state that in France nearly one-half of primiparæ suffer more or less laceration of the perineum. In Vienna's Lying-In Hospital, however, where more than 9,000 women are annually delivered, the proportion of such lacerations, in primiparæ, is six per cent.; it is ten per cent., if incisions with a protected bistoury are considered as lacerations.

But, to obtain such results, I must say that it is not sufficient at the time of birth that the mother should lie on her side, and that the perineum be more or less supported; one needs some practice beside. Thanks to the courtesy of Prof. C. Braun, and his two assistants, Drs. Pawlik and Welponner, I spent three nights in Vienna's Lying-in Hospital, where I performed twenty-one deliveries on the side, under the direction of the hospital midwives, who are very experienced in the matter.

Here is briefly the *modus operandi:* As soon as the head appears at the vulva, the woman is made to lie on her left side, her right leg being raised and held by an assistant. The

accoucheur, standing on the right of the parturient woman, passes his left hand between the woman's thighs, carrying it forward and applying it against the child's head. He supports the perineum with his right hand; but the resistance thus afforded must not be a passive one. He must, on the contrary, during each labor pain, press energetically over the sacro-coccygeal region, and pull as much integument as he can over the child's head. Meanwhile, his left hand steadies the head at the vulva and prevents its coming out under the *influence of uterine contractions*. In the interval between the pains, the head goes back, soon to return again. This forced alternate motion which the head undergoes has for its result the gradual distension and a greater elasticity of the vulva. At last, the head comes out and extension takes place. One must carefully prevent this expulsion from taking place during a uterine contraction, and let the head come out when the pain is nearly over. The perineum must be supported to the end, for the passage of the shoulders is ordinarily more dangerous to the fourchette than that of the head itself. This is the manner, writes Dr. Rendu to the *Lyon Médical*, in which I deliver primiparæ at the clinique of the Charité of Lyon, and Prof. Bouchacourt insists on that method in his lectures before the medical classes.—*Le Médecin Praticien*, March 12.

FOR 5,100 medical students in 1880 in Paris, there were only two chairs of obstetrics, one of theory and one of practice. In London and Manchester there were eighteen for 2,421 students; in Scotland, six chairs, and 1,566 students; in Austria, six chairs, and 1,505 students; in Belgium, four chairs, and 238 students; in Switzerland, three chairs, and 404 students; in Germany, twenty-one chairs, and 4,405 students; in Italy, seventeen chairs, and 3,455 students. While there is thus a chair of obstetrics to 187 students in the rest of Europe, there is but one to 2,550 students in Paris, and only one clinical service for 5,100 students.

Items.

SUGGESTED PROGRESS IN MEDICAL JURISPRUDENCE. — The *St. Louis Medical and Surgical Journal*, recently contained the following suggestions:

1. In certain cases a jury of medical men should be obtained if possible.

2. Medical experts should be summoned and paid by the State, the experts acting as associate judges, assisting the judge to get at the truth.

3. Certain cases might be referred to a commission, fashioned somewhat like the present commissions in lunacy.

4. Experts called by the court to testify, should be paid. In the State of Indiana, the Supreme Court has decided, "that, under the constitution, the State has no right to take a man's particular services without compensation, and that the giving of expert medical testimony is a particular service within the meaning of the constitution."

5. The office of coroner, with the system of coroner's juries now prevailing, should be abolished, as suggested by the President of the New York State Society in 1879, and the system in vogue in Massachusetts be adopted.

4. Life Insurance companies have accumulated a mass of information relative to causes of death, the bearings of certain conditions upon the question of insanity, or of longevity, facts proving or disproving the commission of suicide, etc., etc. At present this information, so useful in the court room, is inaccessible to the majority of physicians. Is it not possible for "medical examiners," individually or conjointly, to bring this material before this society for our edification and learning.

7. Cannot members in good standing in our society be protected against irregulars by enforcing the regulation law? If men are to be permitted to practice by license, is a license granted in some other county good in this county?

8. The few instances in which examinations of supposed invalids have been ordered by the court to be held in the presence of medical men, acting for the court, as "referees and conductors," have been appreciated by the profession interested on both sides of the case: (Proc. Med. Soc. Co. of Kings.)

CRIMINAL LUNATICS.—To place insane convicts in the same institutions, and the same apartments, with insane patients from the best families of our best citizens, is inconsistent with every dictate of propriety. If a wing for insane convicts cannot be provided in connection with the State Prison, a separate building sufficiently secure and suitable for the purpose, should be erected for their accommodation: (Report of the Managers of the New Jersey State Asylum.) The medical superintendent of the same, points out that an ordinary lunatic hospital is not constructed in such a manner as to render it suitable for the detention of persons who add determined criminal propensities to insanity, and that it would destroy its hospital character, to adapt it to that class; and he pleads for the erection of a separate asylum, meeting the requirements of security, and so organized as to supply humane treatment to the really insane, without creating any incentive to convicts to feign insanity.

MENTAL STATE OF GUITEAU.—If the reports are substantially correct, that Guiteau's uncle and two cousins were insane; that he was as a boy ill-balanced, and not to be depended upon; as a youth, irritable, full of vagaries, and without power of concentration in steady work; as a man, possessed with constantly varying extravagant delusions, for which there was no sort of reasonable foundation, with a mind always vacillating between several impracticable projects; that he made a homicidal attack upon his sister with an ax several years ago, without any real provocation; that he has pursued several women with wild plans of marriage; that he has threatened various persons upon trivial grounds; and finally that he supposed he should do the country

a great service, and obey God's direct command, if he could succeed in murdering the head of the government; or even if dissapointment, revenge, political illusions, furnished the chief motives for his act, there can be no kind of doubt that he is a lunatic with delusional insanity of so long standing as to have produced very considerable dementia in a mind of originally bad quality.

The attempt to kill President Garfield differs only in unimportant details from events with which we are made almost daily familiar through the newspapers, and which are sure to occur from time to time so long as irresponsible, insane persons, are allowed to be at liberty. Although there can never be any satisfactory definition of insanity, any absolute criterion of responsibility, or any human means of determining, in doubtful cases, how far the possible self-control had been exercised, it is difficult to avoid the conclusion that all criminals, sane or insane, should be treated from the standpoint of the safety and good of society. The insane asylum is not a safe enough place of custody for the insane of the criminal class; we really need separate criminal lunatic asylums.

It rests with us to estimate the mental condition of the criminal insane, from as nearly a judicial point of view as is possible; it remains for the legislatures and courts, and not for us, to decide what the protection of society demands: (Boston Med. and Surg. Journal.)

CADAVERIC ALKALOIDS.—The very serious question, from the medico-legal point of view, of the spontaneous development of cadaveric alkaloids and their toxic power, was first raised by M. Selmi, of Bologna. After many vicissitudes of alternate favor and discredit, this subject has been revived in France by the researches of MM. Brouardel and Boutmy; in Germany by those of M. Henseman, and in Italy by the *résumé* of all the information on the subject recently published by the original discoverer, M. Selmi. The activity of some of these substances is in no degree inferior to that of the strongest poisons. There are many distinct ptomaïnes, each showing special chemical and physiological characteristics: and they noted the following fact, of

which the importance is clear, that one of these ptomaïnes, extracted from a corpse which had been eighteen months in the Seine, had all the properties of veratrine. Huseman has taken up the study of ptomaïnes from another point of view. He has felt doubtful whether these substances may not account for the poisoning by alimentary substances and putrid infections. The researches of Hoppe-Seyler, Schmidt, Schmideberg, and Panum, have shown, in alimentary substances in a state of putrefaction, the presenc of alkaloids analogous to ptomaïnes. Huseman concluded that, as a matter of fact, the majority of the accidents observed in persons who had partaken of food in a state of commencing putrefaction, are to be imputed to these alkaloids, which are of similiar constitution to ptomaïnes: (Medical News.)

A REUTER'S telegram announces that a whole boat-load of persons out for a picnic at Warrensburg, U. S. A., who partook of lemonade, were poisoned with it; eight of them are reported in the telegram to be dead, and one hundred others in a critical condition. No indication was given of the nature of the poison, and it was very probably a mineral poison. The sources of poison in drinks of the kind are twofold, being either due to incidental impurities in the ingredients—whether acids or salts—or to the apparatus in which they are made, and the consequent production of symptoms of lead poisoning. Some time since, when public attention was directed toward the subject considerably, more or less important quantities of lead were found to be frequently present in the artificial lemonade of commerce, and in other artificial aërated drinks. It is probable in this case, however, that some much more considerable accident had occurred, and that the deaths were due to the substitution of a toxic material for that which was intended. Organic and mineral impurities are not the worst enemies which the consumers of artificial waters have to fear when these waters are not prepared from sources of the utmost purity. There is a well known case in which a shooting party, who partook freely of artificial mineral waters, were, on separating, attacked in their different homes by typhoid fever, traced by the medical officer of health to the organic impurity of the drinks which they had been consuming.

The occurrence of startling incidents such as these serve especially to direct attention to the advantages of an absolutely pure mineral water. One of the first circumstances which gave a great impulse to the popularity which Apollinaris Water has now so largely achieved and so firmly established, was the public statement of an eminent hospital physician in London, a Fellow of the Royal Society, in which he described how he had himself been suffering for many months from chronic symptoms of poisoning, of which he was unable to determine the source until, having identified the cause of his disease as being lead poisoning, he succeeded in tracing it to his habitual use of soda water in syphons. The soda water had taken up enough lead from the fittings of the syphons in which it was enclosed, frequently for long periods before use, to produce the symptoms of chronic lead poisoning.—*London Sanitary Record.*

STEPHENSON COUNTY SOCIETY OF PHYSICIANS AND SURGEONS.—Officers: *President*, L. G. Voigt, M.D.; *Vice President*, B. H. Bradshaw, M.D; *Secretary and Treasurer*, K. T. Stabeck, M.D. *Standing Committees:*—Surgery—B. T. Buckley, M.D.; L. A. Mease, M.D. Obstetrics—B. H. Bradshaw, M.D.; T. L. Carey, M.D. Practice of Medicine—F. W. Hance, M.D.; E. A. Carpenter, M.D. Materia Medica—K. S. Marlin, M.D.; F. A. Darling, M.D. Physiology and Pathology—C. M. Hillebrand, M.D.; C. Brundage, M.D. Dermatology—L. Stoskopf, M.D.; F. C. Winslow, M.D. Publication—B. T. Buckley, M.D.; K. T. Stabeck, M.D.; T. L. Carey, M.D.

The Society is regulated by the code of ethics of the American Medical Association, and the State Medical Society of Illinois. Regular meetings are held quarterly.

SOCIETY MEETINGS.

Chicago Medical Society—Mondays, Oct. 3-17.
West Chicago Medical Society—Mondays, Oct. 10-24.
Biological Society—Wednesday, Oct. 5.

CLINICS.

MONDAY.

Eye and Ear Infirmary—2 p. m., Ophthalmological, by Prof. Holmes; 3 p. m., Otological, by Prof. Jones.
Mercy Hospital—2 p. m., Surgical, by Prof. Andrews.
Woman's Medical College—2 p. m., Dermatological and Venereal, by Prof. Maynard; 3 p. m., Diseases of the Chest, Prof. Ingals.

TUESDAY.

Rush Medical College—3 p. m., Dermatological and Venereal, by Prof. Hyde.
Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.
Mercy Hospital—2 p. m., Medical, by Prof. Quine.

WEDNESDAY.

Chicago Medical College—2 p. m., Eye and Ear, by Prof. Jones.
Rush Medical College—2 p. m., Medical, by Dr. Bridge; 3 p. m., Ophthalmological and Otological, by Prof. Holmes; 3:30 to 4:30 p. m., Diseases of the Chest, by Dr. E. Fletcher Ingals.

THURSDAY.

Chicago Medical College—2 p. m., Gynæcological, by Prof. Jenks.
Rush Medical College—2 p. m., Diseases of Children, by Dr. Knox; 3 p. m., Diseases of the Nervous System, by Prof. Lyman.
Eye and Ear Infirmary—2 p. m., Ophthalmological, by Dr. Hotz.
Woman's Medical College—3 p. m., Surgical, by Prof. Owens.

FRIDAY.

Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.
Mercy Hospital—2 p. m., Medical, by Prof. Davis.

SATURDAY.

Rush Medical College—2 p. m., Surgical, by Prof. Gunn; 3 p. m., Orthopœdic, by Prof. Owens.
Chicago Medical College—2 p. m., Surgical, by Prof. Isham; 3 p. m., Neurological, by Prof. Jewell.
Woman's Medical College—11 a. m., Ophthalmological, by Prof. Montgomery; 2 p. m., Gynæcological, by Prof. Fitch.
Daily Clinics, from 2 to 4 p. m., at the Central Free Dispensary, and at the South Side Dispensary.